

## THE PSYCHOLOGICAL REVIEW.

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### THE PSYCHOLOGY OF INTEREST (I).

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In psychology interest seems to be, in certain respects, like charity, and in pedagogy, too, 'in never faileth.' What makes it useless, however, in many discussions, is the loose and varied manner in which the subject is treated. Whether the discussion is dealing with instincts, impulses, feelings, attention, will, the term interest seems broad enough to cover them all. In pedagogy we have the most beautiful examples of argument in a circle due to similar misconceptions of the situation which has in it interest. "Get attention by making things interesting." So at least we are told. And what is interest? Why, attention; and the formula now resolves itself into the tautology, 'Get attention by making the children attentive.' Or we are told to get the interest in children and a little later this apparently simple process is brought about by making things interesting. Similarly the directions to rouse interest by appealing to the child's curiosity, and the like, mean little more than synonymic substitutions. Rather than throw about these various interpretations in scholastic fashion, which would lead to little more than a development of concepts, or academic definition, I have thought it better to attack the subject *de novo*, examining the leading theories on the subject, and then giving in schematic form the facts in the case as they exist in foundation situations. For this purpose I shall treat interest in its connection with feeling and attention, and then shall consider interest itself in its conative and cognitive aspects.

## I.

In treating of the different contributions made by the various writers to the psychology of interest, it seems proper to begin with Herbart. But at this point a difficulty at once arises. Do we not all know Herbart's theory of interest? Is it not a commonplace among young pedagogical students and is it not shouted from the housetops of learning, as it were? I venture, however, to assert that many who shout loudest for or against Herbart do not fully understand his theory in this connection. Some critics of his system simply pull down a straw man which either they themselves have made, or have found ready-made for that purpose. Herbart's theory, we may say, *schwebt oben in der Luft*, but in the *schweben* its real outlines have become rather indistinct. It seems to me not too much to say that many of the present articles and discussions dealing with Herbart are based on the theories of so-called Herbartian writers, or upon the general opinion of the writers themselves as to what Herbart meant.

Before presenting Herbart's theory of interest as he himself has given it, it may be well to show some of the false conceptions which are prevalent. James tells us that 'the Herbartian doctrine of interest ought not . . . in principle to be reproached with making pedagogy soft.'<sup>1</sup> Dewey's<sup>2</sup> Monograph on Interest is mentioned by De Garmo<sup>3</sup> as 'reconciling interest and effort.' We are also informed 'that the current theory inherited from Herbart, and by him from Rousseau, that everything should be made to depend upon interest, that there should be no *must* in education, seems to the author (J. P. Gordy) thoroughly pernicious.'<sup>4</sup> These are samples of the common opinion. Others are satisfied to stalk about and loudly to proclaim themselves 'Herbartians' because they believe in interest, and this 'interest' is usually a cheap kind of sensationalism or feeling, which, of course, must be reconciled with effort, which gives rise to 'soft pedagogy,' and which, perhaps, may have

<sup>1</sup> *Talks to Teachers*, 1899, p. 111.

<sup>2</sup> *Second Supplement to the Herbart Yearbook*, 1895, revised, 1903.

<sup>3</sup> *Outlines of Educational Doctrine*, 1901, p. 46.

<sup>4</sup> *A Broader Elementary Education*, 1903, preface.

been inherited from Rousseau; but which, none the less, is *not* Herbart.

What then is Herbart's theory? The best explanation of the Herbartian psychology, according to Stout, is to be found in Herbart's own *Psychologie*; and the best exposition of his educational tenets is to be found in his educational writings, and not in the writings of those who explain Herbart according to their own views. Strange to say, Herbart's treatises on psychology deal almost entirely with apperception, without any corresponding treatment of interest. Neither in his *Psychologie als Wissenschaft* nor in his *Lehrbuch* do we find any account. In the latter book we meet with the statement,<sup>1</sup> "Observation and expectation, as the two stages of interest, also belong to the basic concepts of universal pedagogy." This is followed by a statement in parenthesis referring the reader to the *Pädagogik* for a fuller exposition. This reference, by the way, is omitted by Miss Smith in her translation of the *Lehrbuch*. In the *Science of Education*,<sup>2</sup> which contains the fuller discussion of interest referred to, we find interest thus defined: "Interest, which in common with desire, will, and the æsthetic judgment, stands opposed to indifference, is distinguished from these three, in that it neither controls nor disposed of its object, but depends upon it. It is true that we are inwardly active because we are interested, but externally we are passive till the interest passes into desire or volition. It occupies the mean between observation and attainment."<sup>3</sup> Interest is thus, according to Herbart, a feeling which passes or which may pass into desire. Herbart, after this explanation of the term, then skips rapidly on to his seemingly more congenial discussion of the apperceptive systems which give rise to interest, the so-called many-sided interests, *viz.*: (1) empirical, speculative, æsthetic; (2) sympathetic, social, religious.

It is at this point that misinterpretation creeps in. The interests are not these systems of ideas, nor are the systems of

<sup>1</sup> *Works*, V., Hartenstein's edition, 1886, p. 148.

<sup>2</sup> Translation by H. M. and E. Felkin. This translation is the one to which I shall refer.

<sup>3</sup> *Science of Education*, p. 129.

ideas necessarily the result of interest. Rather, interest results from the risings and sinkings of the ideas and systems of ideas. "Interest depends upon concepts, on their opposition and interconnection, on their mode of comprehending various aspects, without being confused with these. . . . Interest attaches itself to the thing, to relationships, not to number or mass."<sup>2</sup> This would make interest, for example, in speculation, result from an interaction of the ideas in the 'speculative' group on some others, and similarly with the rest. According to the Herbartian view, the six-groups, wrongly called 'interests,' are rather apperceptive systems, interest-producing groups. I venture to state that Herbart himself, engrossed in his theory of apperception, introduced interest merely as a pedagogical commonplace, and did not see fully its relation to the apperceptive systems. This is a rather strong statement, but I do not find anything either in his *Psychologie* or in his *Lehrbuch*, or even in his pedagogical writings, to justify any other opinion. Herbart is considered the champion of interest in education because of the so-called six interests. Robbed of all apperceptive terminology, Herbart's interest is simply an Herbartian 'feeling'; and whether the ideas giving rise to the interest are grouped into six classes or not, makes no difference. The interest remains a feeling. It is curious to note that Wundt, who like Herbart, has, in his three bulky volumes, developed the subject of apperception, has also, like Herbart, neglected interest as a psychological topic. Remembering Herbart's real exposition (what there is of it) of interest, one can easily see that there is no contradiction between Herbart's psychology and his pedagogy. The Herbartian interest is not a bit of sensationalism to rouse an apperceptive mass. Sensation or pleasure-pain feeling, as we understand it, would be a *Vorstellung* in the Herbartian sense. Interest is the result of the interaction of a number of ideas or systems of ideas, which persist and remain. To rouse and to develop such ideas and systems of ideas, we do not of necessity need interest. Strength of impression, freshness of susceptibility, degree of opposition present, economy of receptivity, etc., are necessary to rouse and develop the systems

<sup>2</sup> *Science of Education*, p. 134.



of ideas, and not interest.<sup>3</sup>) In teaching, therefore, we must work from within outwards, and not from without inwards, if we are to rouse interest. (We present things which will excite the apperceiving masses to get interest, and vivid impressions, etc., to develop the apperceiving groups. In the interaction of the various ideas on one another, interest arises; the interest does not first cause the apperceiving groups to act.) Interest as feeling in the usual sense will give rise to a contradiction of Herbart's system; but not feeling in the Herbartian sense.

Another point to be emphasized is that Herbart does not say we must depend upon interest in instruction at all times. The groups guaranteeing the interest, and resulting in further interests, may be built up by other means, but when they are there, we are sure of interest. The formation of such interest-producing groups is the be-all and end-all of education. And in this process we need not sugar-coat everything, as the usual interpretation of Herbart would have us believe. We need go but to Herbart himself to show this. I think it advisable, therefore, to give a number of quotations enforcing the view that we should not necessarily pursue soft methods in instruction, and showing that the Herbartian methods are 'soft' only in the minds of those who think so. As regards the government and discipline of children we have the following: "At first, instead of a true will, which renders the child capable of determination, there is only a wild impetuosity, impelling it hither and thither, a principle of disorder, disturbing the plans of the adults, and placing the future personality of the child itself in manifold dangers. This impetuosity must be *subdued*, or the disorderly character will be put down as the fault of the child's guardians." "The first measure that all government has to take is the threat of punishment, and in its use all government runs the danger of striking on one of two rocks: on the one side there are strong natures who despise all threats; . . . on the other there are natures . . . who are too weak to be impressed by threats. . . . This twofold uncertainty of the result cannot be avoided." Finally: "Threats, in case of need en-

<sup>3</sup> *Lehrbuch*, p. 148, and *Outlines of Educational Doctrine*, Ch. IV. (trans. by de Garmo).

forced by compulsion, supervision by persons who are generally cognizant of the dangers to which children are liable — authority and love combined — these powers will pretty easily obtain a certain degree of ascendancy over children." This is hardly as wishy-washy as usually represented. Finally, for the Rousseau heresy, we have: "What most distinctively raises him (Herr Niemeyer) above foreigners and entitles us to think with pride of our nationality, is in my eyes the definite moral tendency of his principles, whereas in the principles of Locke and Rousseau crude impulse holds sway, and, barely mitigated by a highly unstable moral feeling, leads to a superficial sensuous life."<sup>1</sup>

I shall now touch briefly on Volkmann's treatment of interest, and then shall take up Dewey's discussion. In Volkmann we find one or two significant statements and this is all that it is necessary to give. "Since we are accustomed to consider interest as the relation of an idea to the predominant ideation masses of the self (only that interests me by which I can say I am present), so we may consider this expression in the following tautological formulation — We are attentive to everything which interests us." "Where interest is lacking, attention, too, is lacking; where the former is weak, the latter also is weak." As a modern object of instruction, "Instruct in such a manner that an interest is roused which will continue for life."<sup>2</sup>

Volkmann may be considered the last of the orthodox Herbartians, and he adds nothing of importance to the Herbartian doctrine of interest. It is of interest to note that Wundt, who still shows traces of Herbartian influence, has caught the real significance of Herbart's theory. He neglects interest entirely, and develops along more modern lines the theory of apperception. So too, in England, Stout, who shows the influence of Herbart along certain lines, gives a full treatment of apperception and apperceptive systems, but omits entirely in the chapter concerned any mention of interest. His own theory of interest is an entirely separate development, and shows rather the influence of Mill and Stumpf.

<sup>1</sup> *Science of Education*, pp. 95, 97, 102, 251.

<sup>2</sup> *Lehrbuch der Psychologie*, II., 1895, pp. 206, 207.

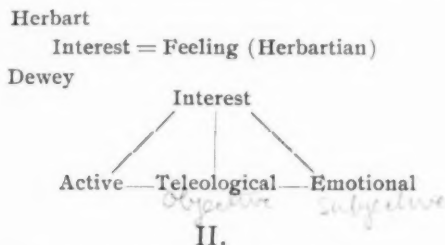
Dewey's account has usually been supposed to expound further, in some manner, Herbart's doctrine of interest, to reconcile the latter theory of interest with the modern theory of effort, and so on. But it may safely be said that the only sense in which Dewey's theory of interest, as such, is Herbartian, is in that of being published in a *Supplement to the Herbart Yearbook*. Dewey is too original a man tamely to follow the usual treatment and end with the excellent though well-worn division of the various interests into empirical, speculative, æsthetic, sympathetic, social and religious. His entire theory is founded on his own principle of activity which forms the basis of most of his psychological and philosophical discussions. Only in the definition of interest as 'consciousness of worth' have we a slight tinge of Herbartian influence. The sections in Dewey's paper dealing with Kant and Herbart, with interest and education, etc., are outside the theory of interest proper which forms the second section of his monograph, *Interest as Related to Will*. Since the monograph is connected with Herbart in current discussion, I add it to the views of Herbart above presented.

"Interest is first active, projective, or repulsive. We take interest. To be interested in any matter is to be actually concerned with it. The mere feeling regarding a subject may be static or inert, but interest is dynamic. Second it is objective. We say a man has many interests to care for or look after. We talk about the range of a man's interests, his business interests, local interests, etc. We identify interests with concerns or affairs. Interest does not end simply in itself, as bare feelings may, but always has some object, end, or aim to which it attaches itself. Third, interest is subjective; it signifies an internal realization or feeling of worth. It has its emotional as well as its active and objective sides. Wherever there is interest, there is response in the way of feeling."<sup>1</sup> Interest may be mediate or immediate. In the latter case the activity exists as an end in itself and constitutes the interest, *c. g.*, play. Where, however, we have a transference of the interest in an end to some means leading to the end, the interest in the means is a mediate one. The closer the connection between the means

<sup>1</sup> *Second Supplement to the Herbart Yearbook*, 1895, revised 1903, p. 13.

and the end, the nearer is the mediate to the immediate interest. Where the means and end are disparate we have effort and drudgery. "Genuine interest . . . is the accompaniment of the identification, through action, of the self with some object or idea, because of the necessity of that object or idea for the maintenance of self-expression. Effort in the sense in which it may be opposed to interest, implies a separation between the self and the fact to be mastered or task to be performed, and sets up an habitual division of activities. Externally, we have mechanical habits with no psychical end or value. Internally, we have random energy or mind-wandering, a sequence of ideas with no end at all, because not brought to a focus in action. Interest in the sense in which it is opposed to effort, means simply an excitation of the sense organ to give pleasure, resulting in strain on one side, and listlessness on the other."<sup>1</sup>

We may summarize the above views in the following schematic form :



I shall now examine the more recent discussions on the subject, some of which have been storm centers in the literature of psychology. Prominent among those who have forced their theories to the front stands Stumpf with his identification of interest, attention and feeling. Due to his influence, the question of the concomitance of interest with attention has been more closely examined, while some psychologists seem to have been led astray by their interpretation of Stumpf's 'feeling.' We must examine the theory of Stumpf to some extent to see exactly what he means and not rest satisfied with placing all our faith in his much-quoted sentence, "Attention is identical with interest, and interest is a feeling. That is all there is to it."<sup>2</sup> Alone,

<sup>1</sup> *Second Supplement to the Herbart Yearbook*, p. 12.

<sup>2</sup> *Tonpsychologie*, 1883, p. 68.

this sentence gives a learned flavor to a discussion on interest, and interpreted in the usual English manner, leads one to believe Stumpf held a theory which he by no means favored. Pursuing the subject further in his *Tonpsychologie*, we find that this interest or attention may be roused by intense or agreeable impressions, or by an impression exciting an image which was of interest on a previous occasion. The strongest stimulus to attention or interest, however, is that which forms the basis of a volition. In such a case the will is not directed upon the attention, but upon some cognitive process, which is based upon an ideational content, and which in turn may become the foundation of a volition. To use Stumpf's words, "The will, therefore, does not produce attention, but it is attention."<sup>3</sup> This makes interest, not only a feeling, but also will. If this is so, then Stumpf's 'feeling' must likewise be a form of will. This gives us some suggestion of his further treatment of interest.

As Stumpf explains in the second volume of his *Tonpsychologie*, his identification of interest with attention caused some criticism, and to meet this he modified his former statements somewhat and gave a more explicit treatment of the subject. As he explains, interest as a term cannot always be substituted for attention when the subject of attention or interest is taken up. But, after making such changes as the language, the context, and the signification of the terms in their contexts demand, we may substitute one term for the other. Thus interest is not 'identical' in the logical sense, but rather 'synonymous' with the expression 'attention-exciting.' In addition, Stumpf further expounds his theory, and we get such a treatment of 'interest as attention and as feeling' as throws sufficient light on the subject to clear any doubts possibly caused by the first seemingly contradictory statements. To examine the characteristics of attention, Stumpf considers it necessary to see what are the characteristics of feeling in general, and then to determine what kind of a 'feeling' attention or interest is. Every feeling, according to Stumpf, is one either of 'Lust' or of 'Unlust,' of pleasure or of pain, of appetite or aversion, *i. e.*, it is either positive or negative. Attention or interest belongs to the positive

<sup>3</sup> *Tonpsychologie*, I., p. 69.

class. It is an interest, a participation-in, a turning-towards something. But the content itself, towards which we are attracted may be agreeable or disagreeable; it matters not which it is. The appetition is not determined by the content as pleasure-pain. "It is rather nothing more than the desire to notice as such, which naturally does not exclude those cases in which an impression, associated with some desire, also excites a desire to observe, discriminate, compare."<sup>1</sup> So, too, interest, 'when it is used as synonymous with attention,'<sup>2</sup> ('identical' now becomes 'synonymous') means not the desire for the agreeable content, *per se*, but rather the desire to know of such content. For example, a critic may be neither attracted nor repelled by an article and still be interested in it. "The article may not have been pleasing to him; but as a matter of fact he may have been pleased to know of it."<sup>3</sup> To this Kohn rather aptly remarks "The critic hates the article, by no means does he desire 'to know of it,' perhaps prefers to take a sleep. The noticing in and of itself is a burden to him. That there is no desire for it therefore, as such, goes without saying."<sup>4</sup> This, however, is by the way.

According to Stumpf, interest is a '*Lust am Bemerken*. More fully, finally, to define the character of this '*Lust*,' it is necessary to turn to Stumpf's discussion of voluntary attention.

<sup>1</sup> *Tonpsychologie*, 1890, II., pp. 279-280.

<sup>2</sup> I translate '*Lust am Bemerken*' as the 'desire to observe or to notice,' and not as a 'feeling of pleasure in observing.' We must take Stumpf in his entire discussion, and not in isolation, before we can determine what he means. In addition to the above, which gives some idea of his meaning, the following may help to throw some light on his meaning of '*Lust*.' He says, "*Die Aufmerksamkeit gehört zweifellos zu den positiven. SIE IST EINE TEILNAHME, EIN INTERESSE, EINE HINWENDUNG ZU ETWAS. Aber der Inhalt selbst, auf den wir merken, kann ein schmerzlicher, hässlicher, verabschauungswürdiger sein. Also ist Aufmerksamkeit nicht ein Lustgefühl, das der Eindruck, auf den wir merken, als solcher mit sich führt. Sie ist vielmehr, wie mir scheint, nichts anderes als die Lust am Bemerken selbst.*" *Tonpsychologie*, II., pp. 279-280. Stumpf's analysis is one of the keenest, but his terminology in this connection seems rather loose. Does he acknowledge this in his expression, "*In terminis libertas, in notionibus unitas*. But still better of course, *In terminis unitas*, also"? II., p. 286. (Small caps are mine.)

<sup>3</sup> *Tonpsychologie*, II., p. 281.

<sup>4</sup> 'Zur Theorie der Aufmerksamkeit.' *Abhandlungen zur Philosophie und ihrer Geschichte*, 1895, p. 32.



Here we are told that "it is nothing other than the will, in so far as it is directed towards an observing. Every feeling of desire which is directed towards an object can pass into a volition as soon as the object seems probable or attainable."<sup>1</sup> Completing now our Stumpfian equation, we have,

$$\text{Interest} = \text{Attention} = \text{Feeling} = \text{Desire} = \text{Will},$$

that is, interest is a feeling of desire which may become an act of volition. It seems, therefore, safe to say that Stumpf in no way identifies interest with feeling as it is usually understood.

A very similar treatment of interest to that of Stumpf is given by Mill and his co-workers in psychology.<sup>2</sup> James has said that the association school has overlooked the problem of attention because it might interfere with the 'smoothness of the tale.' Locke, however, is the first English writer to define attention,<sup>3</sup> and in Mill's *Analysis* we find the first account of attention in which interest and attention are identified. In the *Analysis*, to quote rather fully, we find the following—"A pleasurable or painful sensation is said to fix the attention of the mind. . . . Having a pleasurable or painful sensation, and attending to it, seem not to be two things, but one and the same thing. The feeling a pain is attending to it; and the attending to it is feeling it. The feeling is not one thing, the attention another; the feeling and the attention are the same thing."<sup>1</sup> Where we have a number of ideas or feelings, the stronger may swallow up the weaker. "That the feebler sensation merges itself in the stronger, and is lost in it is matter of common and obvious experience."<sup>1</sup> Attention to a so-called indifferent object is present when this indifferent object is a means to something more pleasurable. The pleasurable idea is associated with it. "The state of consciousness is not an indifferent sensation merely; it is a sensation and an idea in union."<sup>1</sup> Further characterizing attention, J. S. Mill adds in a note, "The essence of attention is that the sensation or thought is, as it were, magnified or strengthened; it becomes more intense as a whole, and at the same time more distinct and

<sup>1</sup> *Toupsychologie*, II., p. 283.

<sup>2</sup> *Principles of Psychology*, I., p. 402.

<sup>3</sup> *Essay*, 1690, Bk. II., ch. XIX., § I., Vol. I., p. 299, Fraser's edition (1894).

definite in its various parts like a visible object when a stronger light is thrown upon it." Objecting to his father's statement that attention is simply the highly pleasurable or painful quality of the sensation, either directly or by association, he adds that in addition such sensation tends, first, to exclude all other sensations from consciousness, and second, to persist in consciousness.<sup>1</sup>

Another name of this pleasurable or painful quality of the sensation or idea is, according to Mill, its interest. Mill is constantly using 'pleasurable' and 'interesting' indifferently. Thus in defining an end which is desired he says: "The end is thus a pleasurable, that is, an interesting idea."<sup>2</sup> And such interest is the same as attention, which again makes interest a feeling. "As the having an interesting sensation, and the attending to it are but two names for the same thing; the having a sensation rendered interesting by association and the attending to it cannot be regarded as two different things. In the first case attention is merely a sensation of a particular kind: in the second it is merely an association of a particular kind."<sup>2</sup> Again — "When the young man in love has the idea of the woman who is the object of his affections, is not attention merely another word for the peculiar nature of the idea? In like manner, in the mind of the man who is to be executed tomorrow, the idea of the terrible event before him is an idea in the very essence of which attention is involved. Attention is but another name for the interesting character of the idea. . . . It remains to expound the case in which an indifferent idea becomes interesting by association. It cannot do so in any other way than those in which it appeared that an indifferent sensation becomes interesting. It may be considered as the cause or the sign of some interesting state of consciousness. When that which is interesting becomes associated with that which is uninteresting so as to form one compound state of consciousness the whole is interesting. An idea, in itself indifferent, associated with interesting ideas, becomes part of a new compound which, as a whole, is interesting; and an interesting idea

<sup>1</sup> *Analysis of the Phenomena of the Human Mind*, 1869, II., pp. 363, 364, and notes 75, I., p. 230, and 66, II., p. 272.

existing and an interesting idea attended to are only two names for the same thing."<sup>1</sup> It might be well here to remark that the account of interest given by James<sup>2</sup> in his *Talks* could have been taken bodily from Mill, so similar are the two theories.

Mill does not give any special treatment of attention and interest, but touches on them in his discussions of feeling, motive, will and the like. From the whole we may summarize his views as follows:

Pleasure-pain quality of the sensation or idea  
 Attention (direct or by association) = interest (direct or by association)

Similar in some respects to the theory of Stumpf, but more closely in accordance with the facts, is the account of interest given by Baldwin in his *Feeling and Will*.<sup>3</sup> "Objects are interesting," according to his view, "only as they affect us or are associated with objects that affect us. And by the phrase 'affect us,' we mean—work some change in the sensibility, which tends, by the law of motor reaction, to realize itself in activity. . . . Now, such affective modifications may come in two ways. The two great stimuli to activity are pleasure and pain on the one hand, and suggestion on the other. Suggestion is passing, shifting, temporary; the interest it arouses is intellectual, temporary interest. . . . The common element, further, is an impulsive element—a tendency element—realizing its object through attention, which is the vehicle of apprehension. Accordingly . . . we may define interest as the *impulse to attend*." Applying his two great laws of habit and accommodation Baldwin finds as the intellectual conditions of interest, first, that 'intellectual repetition diminishes interest,' second, that new relations involving higher integrations, rouse the interest of curiosity, of exploration and discrimination. And on the emotional and active side we have as determinants of interest, first, reference to the self in the form of pleasure-pain, the interest being something over and above the feeling of pleasure-pain. "It arises in connection with the apprehending of the tone and its causes. We would hardly say that an oyster is in-

<sup>1</sup> *Analysis*, II., pp. 361, 367, 368, 369.

<sup>2</sup> *Talks to Teachers*, Ch. X.

<sup>3</sup> Chap. VII., on 'Interest, Reality and Belief.'

terested when a sharp instrument is thrust painfully between his shells. The intrusion affects him, and it is in his interest to avoid it; but it is truer to say that it hurts than that it interests him."<sup>1</sup> Second, the effort expended on a thing gives it additional worth and so conditions our interest in it. In general, interest is the feeling of an impulse to attend to an object; it has a future or prospective reference and is renewed by new impulses, new apperceptive activity. This will give us schematically,

Interest = Ideal feeling = Impulse to attend

Of other writers, Sully touches upon the question of interest, but he adds little to what is given in Mill. In fact, both Mill and Stumpf seem to run through the account given by Sully. He tells us, "When it is said that we attend to what interests us it is meant that we attend when our feelings are touched; that is, to objects or ideas which directly or indirectly excite feeling. We may thus be said to be interested when we experience a sense feeling, and our attention is determined to the object, or to the action that excites this."<sup>2</sup> He also emphasizes the necessity of investigating the relation of interest to attention, quoting Stumpf to this effect: "Stumpf goes so far as to write, 'attention is identical with interest and interest is a feeling.'"<sup>2</sup> Sully hints at what interest might be, but he does not definitely tell us what it is. We are interested when our feelings are touched, but what is interest? Is it a feeling, or is it caused by such feeling? At any rate I shall hazard this equation as based on his account:

Interest = Feeling (?)  
                  /          \  
                Attention.

More explicitly, Stout has given his views on the subject, and in his rather long discussion we again see attention and interest treated together. Stout considers it undeniable "that attention and interest, disagreeable or agreeable, are coincident. We cannot be feeling an interest in an object without attending

<sup>1</sup> *Handbook of Psychology, Feeling and Will*, 1891, pp. 139-146, and Ch. VII.

<sup>2</sup> *The Human Mind*, 1892, I., p. 163, and Note 1, p. 163.

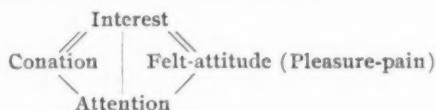
to it, and we cannot be attending to it without feeling some interest in it. Where the interest is very slight and transient, attention is also slight and transient and *vice versa*. But the two always go together. There is, however, a fallacy in the ordinary doctrine of their connection. The assumption that attention *depends* on pleasure-pain seems to have no sufficient basis. The relation is not one of cause and effect. The coincidence is simply due to the fact that interest as felt at any moment is nothing but attention itself, considered in its hedonic aspect. . . . Stumpf, indeed, goes too far when he says 'attention is identical with interest'; but the distinction between them is simply that the word *interest* carries with it a reference to something else as well as to attention as a mode of mental activity; this something else is the pleasure-pain tone of the attention process."<sup>1</sup>

As with Stumpf, so with Stout, we must be careful in interpreting what he means by attention and its hedonic aspect. It seems that in his rather violent effort to break away from the passive pleasure-pain theory of Mill, he has turned completely round and denied it altogether. Everything with Stout is conation and activity in this connection, and one needs but to skip through his book to verify this. For example he says: "There can be no such thing as purely passive consciousness. This means (1) that a total psychosis or state of mind can never be a state of complete inaction; (2) that no special content of consciousness entering into the composition of a total psychosis can exist apart from its relation to mental activity."<sup>2</sup> To make doubly sure that he is not misunderstood, Stout further says: "The process of consciousness is, as such, a felt process";<sup>2</sup> *i. e.*, we know that consciousness is active not only through reflection, but we also feel its activity during its existing moments. Attention is therefore mental activity, a conative tendency, a striving towards an end, and felt as active, while the concomitant feeling of pleasure-pain is due to the furthering or thwarting of our practical aims. "The pleasant or painful feelings involved, as springing out of the furtherance or thwarting of our

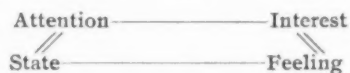
<sup>1</sup> *Analytic Psychology*, 1896, I., pp. 224-225, and also Ch. III. of Bk. II.

practical aims, is coincident with the apprehension of the object and its significance for us."<sup>1</sup>

In his *Manual* Stout gives a similar theory of interest. Conative continuity and continuity of interest are considered as the same. "It is clear, then," he writes, "that continuity of interest is more or less independent of direct proximity in time. This kind of continuity is essentially connected with mental activity in the strict sense, with the striving, conative, appetitive side of our nature. Its general condition is that the successive phases of a conscious process shall constitute a movement towards an end-state or terminus."<sup>2</sup> In almost every mention of 'conation,' Stout uses the term of 'interest' as synonymous. His idea of the matter may be represented as follows, portion of the diagram being taken from his *Groundwork*:<sup>2</sup>



The concomitance of interest with attention is also emphasized by Titchener who says, "When we say an 'interesting' thing catches the attention we are really speaking tautologically. A thing is 'interesting' when it is 'a thing to be attended to.' It is not that the pleasantness or unpleasantness comes first, and that we then attend to the impression; the two parts of our experience, the affective and the attentive, are simultaneous. In popular parlance, we attend because the thing is interesting; in psychological language the interest and the attention are two sides of the same experience."<sup>3</sup> According to Titchener, therefore, we have the following:



Among the other psychologists we do not find so full an account of interest given, though the subject is more or less fully touched upon. James tells us that consciousness "is always

<sup>1</sup> *Analytic Psychology*, I., pp. 166, 168, 225.

<sup>2</sup> *Manual of Psychology*, second edition, 1901, p. 81, and *Groundwork of Psychology*, 1903, p. 19.

<sup>3</sup> *Outline of Psychology*, 1901, p. 143.



interested more in one part of its object than in another";<sup>1</sup> that "interest alone gives shade, background and foreground — intelligible perspective, in a word,"<sup>1</sup> and that "the things to which we attend are said to interest us. Our interest in them is supposed to be the cause of our attending."<sup>1</sup> James also refers to interest as a selecting principle.<sup>1</sup> In his *Talks* he again takes up the subject. The native interests of children lie altogether in the sphere of sensation."<sup>2</sup> "*Any object not interesting in itself may become interesting through becoming associated with an object in which an interest already exists. The two associated objects grow, as it were, together; the interesting portion sheds its quality over the whole; and thus things not interesting in their own right borrow an interest which becomes as real and as strong as that of any natively interesting thing.*"<sup>2</sup> This statement would fit much better in Mill's Analysis than in anything that James has written; which implies, of course, not that James is bad, but that Mill is good.

We shall end our historical survey by rapidly giving what the other psychologists treating of interest have given. Ladd considers interest a feeling. As he says, "the question of gaining attention, is, in general a question of exciting some kind of feeling. In fact, the power which different objects of sense or ideas have to get attention in that 'struggle for existence' which takes place in the stream of mental human life is all summed up in that one word indicative of feeling, 'interest.' It is acknowledged by all that different minds have very different interests. But with all this great diversity of particular interests it is also the acknowledged universal rule that men attend with ease and effectiveness to what interests them, but only with difficulty and reluctance, or not at all, to what does not interest them." "This, however, is far from warranting us in saying, as Stumpf does, 'Attention is identical with interest, and interest is a feeling.'"<sup>3</sup> Finally Miss Calkins, assuming (1) Stumpf's dictum, '*Aufmerksamkeit ist identisch mit Interesse, u. s. w.,*'

<sup>1</sup> *Principles of Psychology*, 1890, I., pp. 284, 402, 416, 515, 572, 594; II., p. 344.

<sup>2</sup> *Talks to Teachers*, 1899, pp. 92, 94.

<sup>3</sup> *Psychology, Descriptive and Explanatory*, 1894, p. 79.

and (2) combining it with the formula of attention given by Titchener and by James, attention as a state of clearness, gets as a description of attention and therefore of interest which she identifies with attention, the following: Attention or interest is the 'feeling of clearness' which is present in such moments.<sup>1</sup> It is evident that these two will hardly mix if we take Stumpf's *Gefühl* in its actual signification.

(To be concluded.)

<sup>1</sup>*Introduction to Psychology*, 1901, pp. 137, 140, 488.

## ARE THERE HYPNOTIC HALLUCINATIONS ?<sup>1</sup>

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When we first formed our acquaintance with hypnotic phenomena, we had many reports of the wonderful feats possible in the hypnotic state. Among the many marvels hypnotic and post-hypnotic hallucinations induced by suggestion occupied a prominent place. A good many of such hypnotic studies have been the result of amusement and at best of wonder. If it be true as Plato and Aristotle tell us that the origin of science is in wonder, all that is well and good as a beginning. When however we find that the beginning persists, when we find that the apparently precocious baby does not reach its adolescence and manhood, we begin to wonder what the trouble may possibly be, and whether it is not a case of mental defectiveness. When we find that as the literature of the subject grows the same state of blank wonder still persists; when we find that hallucinations induced by suggestion are described in all their minutiae without the least critical psychological analysis of the phenomena, it is time to ask whether the preliminary stage of hypnotic marvels and mysteries has not lasted too long. It is well to pause and ask the question: "Are there any hallucinations hypnotically suggested? Does the subject really experience the hallucinations?" We rarely find in the whole literature of the subject that any of the writers should even as much as refer to the question of the validity of the hypnotic hallucinations. The hypnotic subject accepts the experimenter's suggestion and the experimenter takes the subject's honest word on faith. The trust is mutual. He however who has devoted time and reflection to the matter realizes that the introspective account of the hypnotic subject must be taken *cum grano salis*. First, because one rarely finds a subject who is able and trustworthy to give an introspec-

<sup>1</sup> Read before the American Psychological Association, December, 1905.

tive account; and second, one must always remember the training and extreme suggestibility of the subject, a suggestibility which makes the subject insist on what the experimenter suggests to him, no matter whether he really experiences it or not. This is the dangerous pitfall of hypnosis, into which many an investigator in this field has fallen. We cannot possibly base our scientific generalizations on the insight and 'psychologizing' of one hypnotic case, nor can we accept universally the statements of the subject on the ground that the latter is of an exemplary conscientious character. One must be constantly on his guard, have many cases and keep on constantly sifting his material and that with a good deal of scepticism. One must watch his subjects or patients very closely, always have his suspicions, compare their statements with one another and especially with those of the same patient in various states.

In my experience of many years with hypnotic subjects, some of whom went into a deep somnambulistic state, a doubt gradually began to arise in my mind as to the validity of the hypnotic hallucination. When I came to devote my time to studies of cases afflicted with hallucinations, the doubt grew stronger and almost became a certainty. I could not help reaching the conclusion that the hallucinations hypnotically suggested are not genuine. In other words, facts lead me to think that there is no hypnotic hallucination in the strict sense of that word.

Before we proceed with our discussion it may be well to give an analysis, however brief, of the percept of hallucination, and then compare the latter with experiments and observations of hypnotic and post-hypnotic hallucinations.

We may begin with the percept and its elements. In looking at the vase before me I see its beautiful tints, its rounded shape, its heavy pedestal with its rough curves, its solidity, weight, brittleness and other experiences which go to make up the perception of the vase. Now, the visual elements are given directly by the visual perceptive experience; but whence come the seemingly direct experiences of weight, heaviness, roughness, smoothness, and others of the like kind? They are evidently derived from other senses. The whole perceptive experience is of a visual character. We take in the whole with

our eye. In the organic structure of the percept then, besides the experiences directly given by the stimulated sense-organ, there are other experiences, sensory in character, indirectly given, and coming from other sense organs which are not directly stimulated.

The percept is a complicated dynamic product, and its elementary processes are never derived from one isolated domain of sensory experience. The activity of all the sensory domains coöperates in the total result of an apparently simple percept. Along with sensory processes directly stimulated, a mass of other sensory processes become organized and help to contribute to the total result. The direct sensory elements are termed by me *primary sensory elements*; the indirectly given experiences are termed *secondary sensory elements*. The secondary sensory elements may be figuratively said to cluster round the primary sensory elements as their nucleus.

The whole perceptual experience is tinged by the character of the primary elements which constitute the guiding nucleus, so to say. Thus, where the primary sensory elements are visual, the whole mass, no matter from what domain the sensory experiences are derived, appears under the form of the visual sense, and the percept is a visual percept. While the primary sensory elements form, so to say, the dynamic center of the total perceptual experience, the secondary sensory elements mainly constitute its content. Both primary and secondary elements are sensory and are induced peripherally; the primary directly, the secondary indirectly. The percept then is sensory and is constituted by primary sensory elements, or primary sensations, and by secondary sensory elements, or secondary sensations.

The character of the secondary sensory elements stands out clear and independent in the phenomena of synæsthesia, of secondary sensations. In synæsthesia we have a sensation of one sense organ followed, without an intermediary direct stimulation, by a sensation coming from another sense organ. Thus, when a sensation of light instead of giving rise to a subsequent idea gives rise to a sensation of sound for instance, we have the phenomenon of secondary sensation. Here the secondary sen-

sations stand out free and distinct, but they are really always present in our ordinary perceptive experiences as bound up secondary sensory elements, as secondary sensations grouped around primary sensations.

When the phenomena of synæsthesia were first brought to the notice of the scientific world, they were regarded as abnormal and exceptional, and only present in special pathological cases. Soon however their field became widened, and they were found not only in the insane and degenerate, but in many persons otherwise perfectly normal. We find now that we must further widen the field of secondary sensory elements and, instead of regarding them as a freak of nature existing under highly artificial conditions, we must put them at the very foundation of the process of perception.

Secondary sensations are at the basis of perception. We have become so accustomed to them that we simply disregard them. When, however, the conditions change, when the secondary sensations stand out by themselves, isolated from the primary nuclear elements with which they are usually organically synthesized into a whole, into a percept, when they become dissociated, it is only then that we become conscious of them directly and declare them as abnormal. Secondary sensations are always present in every act of perception; in fact they form the main content of our perceptual activity, only we are not conscious of them and it requires a special analysis to reveal them. Secondary sensations *per se* are not something abnormal — just as hydrogen present in the water we drink or the oxygen present in the air we breathe are not newly created elements, — it only requires an analysis to discover them. If there be any abnormality about secondary sensations, it is not in the elements themselves, but rather in the fact of their dissociation from the primary nuclear elements.

Now when the secondary sensory elements come to the foreground and stand out clearly in consciousness, a full-fledged hallucination arises. In the phenomena of synæsthesia we have hallucinations in the simplest form, inasmuch as only isolated secondary sensory elements dissociated from their active primary central elements stand out in the foreground of consciousness.



This very simplification however of hallucinations reveals their inner character. The most complex hallucinations are only complex compounds, so to say, of secondary sensory elements. Hallucinations are not anything mysterious, different from what we find in the normal ordinary processes of perception; they are of the same character and have the same elements in their constitution as those of perception. Both hallucinations and percepts have the same secondary as well as primary elements. The difference between hallucinations and percepts is only one of relationship, of rearrangement of elements, primary and secondary. When *secondary sensory elements become under conditions of dissociation dynamically active in the focus of consciousness we have hallucinations*.<sup>1</sup>

From this standpoint we can well understand why a hallucination, like a percept has all the attributes of external reality. A hallucination is not any more mysterious and wonderful than a percept is. We do not recognize the humdrum percept, when it appears in the guise of a hallucination, and we regard it as some strange visitant coming from a central, from some super-sensory universe. Hallucinations, like percepts, are constituted of primary and especially of secondary sensory elements, and like percepts, hallucinations too are induced peripherally.

Now how is it with suggested<sup>2</sup> or hypnotic hallucinations? Do we find in hypnotic or suggested hallucinations, as in the case of hallucinations in general, the requisite primary and secondary sensory elements directly and indirectly induced? Binet makes an attempt to establish a peripheral stimulus in the case of hypnotic hallucinations, claiming that there is a *point de repère*, a kind of a peg, on which the hypnotic hallucination is hung. It is questionable whether Binet himself still maintains this position. However the case may be, this position is hardly

<sup>1</sup> For a full account see my paper 'An Inquiry into the Nature of Hallucinations,' THE PSYCHOLOGICAL REVIEW for January and March, 1904; also the excellent paper on 'Hallucinations' published by my friend and former associate, Dr. Wm. A. White, superintendent of the Government Hospital for the Insane, Washington, D. C., in *The Journal of Nervous and Mental Diseases* for November, 1904.

<sup>2</sup> Throughout this paper I use the term 'suggested hallucination' to indicate the character and origin of the latter. The term seems to me convenient and may prove acceptable.

tenable when confronted with facts. Hypnotic hallucinations may develop without any peg and prop. Furthermore, granted even that now and then such a peg could be discovered, and that the alleged hypnotic hallucination develops more easily when such a peg is furnished, still the fact remains that even in such cases the peg is altogether insignificant, that it is altogether out of proportion and relation to the suggested hallucination, and that on the same peg all kinds of hallucinations can be hung, and that finally it can be fully dispensed with. All this would go to show that the peg, as such, is of no consequence, and is really more of the nature of an emphatic suggestion for the development of the alleged hypnotic or post-hypnotic hallucinations.

The arbitrariness of the hypnotic hallucinations, showing that the whole thing is simply a matter of representations, or of what the patient happens to think at that particular moment, is well brought out in the following experiments: Mr. F. is put into a hypnotic state and a post-hypnotic suggestion is given to him that he shall see a watch. On awakening he claims he sees a watch. The eyeball is then displaced, the watch is also displaced; now when the eyeball returns to its normal condition we should expect that the hallucinatory watch would return to its former place; but no, the watch is not perceived in its previous place, — it appears in a displaced position. The hallucinatory watch could thus be displaced any distance from its original position. The patient evidently did not see anything, but simply supplied from his stock of knowledge as to how a seen watch would appear under such conditions, and he omitted to notice the fact that with the normal position of the eye the watch should once more return to its former position. Such inconsistencies are often found in hypnosis. More intelligent and better informed patients would reason out the matter differently and would give different results. If the subject knows of contrast colors and if a color is suggested to him he will without fail see such contrast colors. If his eyes have been fixed on some hallucinatory color, such as red, for instance, he will even give you a detailed account of the green he sees, but if he does not know anything of the

effects of contrast colors no amount of fixation on hallucinatory colors will bring out the least contrast effects. The reason is the patient does not know anything about it and cannot think of it. We tried to mix by suggestion different hallucinatory colors, and as long as he knew nothing of the real results his replies were uniformly wrong; no sooner did he find out what the right mixture *should* be than he gave correct results. The hypnotic subject really does not perceive anything; he simply tells to the best of his abilities what he believes he ought to see under the given conditions.

It is extremely interesting to make one experiment which gives an insight into the alleged suggested hallucination and shows its fictitious character. The experiment succeeds best when the subject is unprepared and is taken off his guard. I have tried it in various cases and have had uniform results. A suggestion is given to the subject to see a watch, say, on awakening. When he awakens, the watch of course is claimed to be seen in a kind of perfunctory manner. If now another watch is put near the hallucinatory watch, the real watch is not taken notice of; it is absolutely ignored as if it did not exist. If his attention is drawn to the real watch the subject still continues to treat the real watch as unreal, and the suggested hallucinatory watch as the *only real one*. It is evident that in his honest zeal to carry out the suggestion he overdoes the matter and thus clearly reveals the fictitious character of his alleged hallucination, which he in fact does not experience. If now we give him the benefit of the doubt and tell the subject, when in the hypnotic condition, that when he wakes he will see two watches, thus calling his attention by suggestion equally to both watches, one as much as the other, on awakening he still ignores the real watch and his whole attention is occupied with the hallucinatory watch. The subject simply *overacts*. He is so anxious to carry out your suggestion and oblige you. If we now try to test the matter by choice and ask him which of the two watches he prefers to have, he unhesitatingly points to the hallucinatory watch. When asked the reason, he replies almost anything that may at that moment occur to him; such for instance as that the hallucinatory watch is newer or bigger,

or any old thing he may happen to think of, no matter how absurd the reason is. In his eagerness to carry out the suggestion and to show the reality of the hallucinatory watch he will choose the hallucinatory in preference to the real watch. The subject in short does everything in his power to convince us of the reality of his alleged hallucination, and in his eagerness he overdoes things, thus clearly revealing the fact that he really does not perceive the hallucinatory object. The so-called hypnotic or post-hypnotic hallucination is really not a hallucination and should not be taken as such; the *suggested hallucination is more of the character of a delusion.*

To take a couple more of my experiments with cases of hypnotic subjects: H. R. goes into a deep somnambulistic state. I carried out on H. R. a series of experiments in color hallucinations. The results were far from being uniform. To take an example: He was given a suggestion to see red; he saw it, of course. He was then told to look at it and tell me what he saw. At first he answered at random; he saw the chair, the table, the books, and so on. When he found out that color was wanted he obliged me with that. All kinds of contrast colors, white, blue, yellow, orange and brown were given. No sooner did he find out the approved color than he saw it and stuck to it afterwards. Similar results I had in the case of mixture of hallucinatory colors. Red and green, for instance, gave all kinds of results but the right one. No sooner was the right color hit and felt by the subject that the experimenter approved of the guess, than he kept on 'seeing' it without any further modification.

All through these experiments a good deal of fishing was done by the subject, and this fishing was probably the most instructive part of the experiment. On the whole, I must say that the statements of the hypnotic subject should be treated with extreme circumspection. I often wondered which of the two is the greater dupe, the subject or the hypnotizer.

On one occasion I suggested to H. R. to see a hallucinatory pencil; he saw it, of course. He was given the suggestion to insert the pencil in water; he did so. 'What happened to the pencil?' I asked. 'It turned red,' he replied. When, however,

he found out that the experimenter had in mind the refraction of the pencil, then his pencil behaved accordingly. The subject, especially the good one, the one who is capable of good training, is eagerly on the lookout for the slightest wish and caprice of his manager, and is always fishing for the best way of doing it, of carrying out those wishes to the delight of the hypnotizer.

In cases of primary dementia or dementia præcox and katatonia under my observation and experimentation, now being carried on and soon to be published, I found hallucinatory states strikingly like those of hypnosis. Thus in one of my cases of katatonia, Q. M., the patient could readily be made to see all kinds of hallucinations, no matter how incongruent. The patient could see a 'four-horned chicken with toes on its tail'; 'a cat with two legs and wings'; 'a tree with feet, eyes and wings,' and other visions of like kind. To demonstrate their reality the patient actually drew these visions. (Similar experiments were carried out by me on hypnotic subjects.) The patient could be made to see lions, wolves, elephants and tigers, and was no more afraid of them than the hypnotic subjects are in similar conditions. The fact is that neither the demented nor the hypnotic subjects actually experience the suggested hallucinations.

Experiments, therefore, point to the fact that suggested hallucinations, hypnotic and post-hypnotic, are purely ideational and closely conform to the course of associative, ideational or representative activity. In other words, the subject does not really perceive the suggested hallucinatory object. He simply thinks of it. The subject to whom I gave the suggestion of seeing the watch no more saw the hallucinatory watch than I saw it. He *thought* of the watch, and he *claimed* he saw it and acted as if he did see it. Furthermore, he was anxious to carry out fully my suggestion to the best of his abilities, and persuade me that he really did see an actual watch *more real than a real* watch. The alleged hypnotic or post-hypnotic hallucination is not at all of the nature of a hallucination, it is a *delusion*. All we do by such a suggestion is to act on the subject's belief. The subject believes that he perceives, and he, in his turn, as

one under a delusion, tries to convince us of the reality of his belief; and I must say that he is quite successful in imparting this delusional belief to the experimenter himself thus unconsciously, but ironically none the less, repaying his deluder in the same coin.

One of the special characteristics of hallucinations, as of percepts in general, is that of *reality* or of *externality*. The hallucination, like the percept, during the time it is experienced, is clothed in the full garb of external reality. The hallucination is regarded by the percipient as an external object of perception. Hence he who suffers from hallucinations experiences them as he does any normal sensory percept, and, unless he learns in other ways the hallucinatory character of his perception, he reacts to it as he would to any of his normal percepts of external reality. Now if we closely watch the subject possessed by the alleged hypnotic or post-hypnotic hallucination, we find that the inmost character of externality is sadly lacking. The sensory-motor reaction is by no means the one produced by the corresponding sensory percept. After all, the best criterion of an experienced percept, especially if it be that of another organic living being, is the total motor reaction. The proof of the pudding is in the eating. The subject does not react to the 'suggested' pudding as if it were worth the eating. The reaction is by no means the one called out by the perception of an external object regarded for the time being by the percipient as actually present, no matter whether that presence be right or wrong, whether it be hallucinatory or not. The suggested hallucination calls forth a reaction, if there is any at all, of a very weak character and disproportionate to the supposed stimulating presence of the hallucinatory object. The motor reaction is such a one as is called out by a representation, by a thought of the object, but not by an actual perceptive experience of an external object, as it is in the case of an actual hallucination. In hallucinations of ghosts or of tigers, for instance, the patient actually perceives, sees the ghosts, the tigers, the serpents; but in suggested hallucinations the subject sees the suggested objects no more than we do when we talk about ghosts, tigers and serpents, or when we suggest them to the subject.



The subject thinks of the suggested hallucinations and acts as if he perceived them. In fact, the most we can do is to create in him a belief in the supposed presence of the suggested hallucinatory object. *The subject is not in the psychic condition characteristic of hallucination, but he is in the mental state characteristic of delusion.*

The most we can say of hypnotic, post-hypnotic, or suggested hallucinations is that they are saturated, so to say, with the belief in the supposed presence or existence of the object suggested, somewhat in the same way as the child believes in Santa Claus, or as the school boy believes in Washington, or as we believe in the existence of Julius Caesar. The belief, however, is not of the vital over-bubbling stimulating effect given by a direct perception of an external object, true or hallucinatory, but is one essentially representative in character. And that is all that we can claim for the potency of the hypnotic state and efficacy of suggestion.

Hypnosis with its allied states can modify, undermine, create belief, and important modifications can be induced in the total mass of representative life activity. Associations and dissociations can be brought about in the dynamic processes of representations or ideas, but we cannot create objective sensory miracles. If faith and ideas move the world, as they most certainly do, we have in our hands a powerful instrument, which if intelligently used may prove of the greatest benefit to civilization and humanity. We should not, however, delude ourselves and ascribe to it mysterious magic virtues. The hypnotic state with its abnormally increased suggestibility can give rise to belief, to new associations and dissociations of ideas, but by no legerdemain can it produce, without the intermediacy of peripheral physiological processes, the faintest sensory element. Faith may move mountains, but it cannot create the minutest particle of dust, nor can suggestion create the most insignificant sensation.

We should not delude ourselves as to the power of suggestion, even if it be in the hypnotic state. A suggestion even in a hypnotic state, however deep, can do no more than a very vivid persistent idea can do in the waking state. An ideational proc-

ess can give rise to motor and possibly to some glandular changes but no mental magic will ever make of it an external stimulus capable of giving rise to peripheral physiological processes resulting in a sensation with the cumulative effects ending in perception. Suggestion cannot cause an amputated leg to grow, nor is suggestion a reliable antitoxin in infectious diseases, nor is it regarded as an efficacious antiseptic; it can hardly be credited with the power of destruction of the minutest bacterium, nor is suggestion regarded as possessing the medicinal virtues of regenerating a single destroyed neuron. Why then should we be so credulous as to endow suggestion with the wonderful and mysterious qualities of producing sensations and percepts without their adequate complex peripheral physiological processes?

As far as our present knowledge of facts goes we are only justified in saying that the efficacy of the hypnotic state with its greatly increased suggestibility is limited entirely to ideational processes, to their integration and disintegration and that it can do no more than can be effected by a very vivid idea under the most favorable conditions. But are we justified in claiming that ideas however vivid can become sensations and perceptions? No more than our ideas of vapor can become the power of steam. We may as well claim that our idea, say of red, provided it is 'strong' enough, may give rise not only to the peripheral, physiological, sensory processes, but also to the requisite physical processes, to ether vibrations of color red, and thus influence by a backward 'reverse current,' so to say, the sense organs and mind of other persons.

We may arrive at that 'scientific' speculation of telepathic power possessed by our minds to impress the 'Universal Ether,' and imprint on it our thoughts and wishes and sensations and it simply remains for other men or 'sensitive mediums' to breathe in or take in the impressions that flood the ethereal universe.

There is not a particle of evidence that ideas, however vivid, may become 'centrally' transformed into sensations. The idea of musk does not smell; the idea of white does not shine, and the idea of sound does not ring. The suggested hypnotic and post-hypnotic hallucinations, along with the alleged central hal-

lucinations and other notions of like kind belong to the general category of psychological and psychopathological fallacies. It savors somewhat of the mediæval alchemists' belief that gold could be refined out of any old rubbish. Suggestion reminds one of the magic powers of the philosopher's stone,—its touch can convert the base metal of fictitious ideas into the pure gold of sensory experience. The wonder is that the world is not one large asylum for ideas to play gambols in, and raise havoc with all our sensory experience, and make us suffer from all kinds and forms of hallucinations, inasmuch as a high-pitched ideational activity would give rise to the same sensory elements and consequent perceptions as do peripheral stimuli and physiological processes. Our ideas would be regarded as realities and our great expectations as actualities. It is agreed on all hands that no sensation can be transformed into another even if they both belong to the same domain—the sensation of yellow cannot become orange much less that of sound, for instance, because they are qualitatively different. How then can we maintain the untenable position that ideas, no matter how intense and vivid, can ever become sensations, percepts, can ever form sensory and perceptive experiences, even if they be hallucinatory in character.

The validity of hypnotic hallucinations has passed unchallenged, because of the dubious assumption of the central origin of hallucinations, an assumption still current among psychologists and especially among psychiatrists who still pin their faith to 'images and idols' and accept uncritically the introspective lucubrations of insane and demented. Although the introspective account of the hypnotic subject is far more trustworthy and valid than that of the insane and demented, it should be taken with the utmost caution and should not be too credulously accepted on its face value.

If we eliminate then the psychopathological fallacy of central transformation of ideas into sensations and percepts, we clearly realize the flimsiness, the spurious character of suggested, or hypnotic hallucinations. *We have first of all to prove that the subject actually experiences the suggested hallucinations.* We should not be blinded by a too devout worship of 'central images,' but should pay more attention to facts, to the actual mental

condition of the hypnotic subject. The fact that the introspective account given by the subject confirms most emphatically the presence of an actual hallucination does not count in this case. The introspective account is just the one that is the *least reliable* in such cases because of the untrustworthy suggestible nature of the whole state, and as such should if possible be avoided, especially in cases of long standing in which suggestibility has been trained to its utmost. We must always keep in mind the highly suggestible character of the hypnotic subject, and that from the very nature of his state of suggestibility he will stick to the apparently objective description of a purely ideational experience. We must remember that the suggested hallucination is given in terms of objective perceptual experience and that the more effective the suggestion is, the more suggestible the patient is, the more will he insist in his introspective account on objective description of his experience given to him in the suggestion by the experimenter. The so-called *hypnotic hallucination is an artifact elaborated by both parties in the experiment*,—it is a kind of an unconscious collusion formed between the experimenter and the subject.

In order to find out the delusional nature of the suggested hallucination it is well to begin with an intelligent trustworthy subject who has no knowledge of the marvels and mysteries of the hypnotic state and no training has yet been permitted to effect by means of the cumulative effect of suggestion the highly artificial results which destroy the value of many a valuable case. Now if we take a fresh case with no mystical humbug about it we meet with results far different from those which are usually described and reported. Of the various cases under my observation and experimentation I may take the case of one patient who went into a deep hypnotic state. To quote from my notes: "Mr. N. goes into deep trance. While Mr. N. is in the hypnotic trance I suggest to him that as soon as he wakes he shall go to a jar full of water and look into it and see a series of scenes from his former life. On awakening and hearing the signal he goes to the jar, looks into it, and begins to recite the scenes or the events of his former life. I suddenly stopped him and asked him: Do you see all that in the jar? No, he replied, I see it in

my mind; I have it all in my mind." The subject did not really see it; he simply thought of it. The subject is hypnotized again; the suggestion of hallucination is enforced. On being awakened, when the signal is given he goes to the jar, looks into it, and begins once more to recite his supposed visions. Do you see them in the jar? I ask again. I do not know, he answers, whether I see them in the jar or in my mind; it is hard to tell. Evidently my succeeding suggestion has brought about some hesitation in his belief or attitude as to the pure ideational character of his supposed visions. He would not have hesitated in his statement as to the whereabouts of the alleged hallucinatory objects had he perceived the actual external objects as is the case in actual hallucinations. The hesitation is also instructive from another standpoint, namely, the training which the subject gets by the form and insistence of the suggestions given to him. The patient was just on the point of giving way to the suggestion of objectivity of pure ideational experiences; a few more experiments and suggestions and he would have given a full description of a suggested hallucination in its full perceptual objective glory, so delightful to the heart of most experimenters. The simple truth of the whole matter is that the patient had not the least perceptual experience of the objects suggested by the alleged hallucination; he did not see anything, he did not see the scenes anywhere, he only thought of them, he simply remembered them, possibly very vividly, but still they were only thoughts, memories, and not perceptions, not actual visions.

I could similarly bring from records a number of other cases under my observation, cases in which the suggested hallucinations were at first regarded as unsuccessful and only after some repetition have the hallucinations apparently become fully developed. Thus in some of the cases put into hypnosis for the first time the subjects declared on awakening that they did not see anything, but that the thought of the suggested object came to their minds. What really happened was that the subjects not as yet trained by a whole course of 'suggestive treatment' gave me a real account of what was actually taking place in their minds. Such results are usually regarded as unsuccessful.

ful suggestions. As a matter of fact, it is such failures that are really successful and that give the actual state of mind, while the *successful suggested hallucinations are artifacts*.

Dr. R. D., with whom I carry on extensive series of experiments, goes into a very deep somnambulistic state. He is an excellent visualizer and takes readily visual hallucinations. Being a trained physician and psychiatrist the subject's account is all the more valuable.<sup>1</sup> Now R. D. describes his hallucinations as 'mental pictures,' as 'auditory memories,' which 'lack exteriority, are not located in space.' He aptly characterizes his hallucinations visual, auditory, and others, as '*fixed ideas*.'

Mr. M. goes into deep hypnosis. When in one of the deep trance-states a suggestion is given to him that on awakening he shall see a watch. When awake he claimed he saw a watch. He was asked: 'Do you really see it?' He replied 'Yes.' The interesting point here was the fact that the subject did not even look in the direction where the suggested hallucinatory watch was supposed to be placed and where he himself claimed that the watch was located. When tested by automatic writing the hand wrote: 'Yes, I see the watch.' The subconscious then was also under the influence of the suggested hallucination. This point it is well to bear in mind.

Rehypnotized, and suggested that on awakening he would see *two* watches. One was a real silver watch and the other was hallucinatory. The subject claimed he saw both, but he handled the hallucinatory one, and when asked which of the two he would prefer he pointed to the hallucinatory watch. When asked why, he replied that the suggested watch was bigger. He was really indifferent to the chosen watch and paid no further attention to it as if it did not exist for him.

He was again put into the hypnotic state and was suggested to see a flower. On awakening he claimed he saw a flower and smelled it in an indifferent, perfunctory fashion. The subconscious was then tested by automatic writing and the writing was to the effect that he saw it: 'I see a flower.' The subconscious then had also the same hallucination. A series of similar experiments was carried out with the same results. *The subcon-*

<sup>1</sup> The experiments will be published in full.



*scious claimed in automatic writing that the suggested hallucination was real.*

The subject was again put into hypnosis and was given the suggestion that he would see a watch on awakening, but here I made some modification. 'When you wake up you will be sure to see a watch,' I said, emphatically. "Look here; I want you to write what you really see and not what you do not see." When awake he saw a watch, but he immediately wrote: 'I do not see anything.' *Here the subconscious disclaimed the suggested hallucinations which it had claimed and insisted on before.*

Rehypnotized, and was given the suggestion that on awakening he would see three watches. He was awakened and a real silver watch was put before him; the other two were hallucinatory. He claimed he saw all three. Meanwhile, in automatic writing he wrote: 'One silver watch, real, the other golden, not real; nothing there.' A series of similar experiments was made and with the same results. *The automatic writing disclaimed the hallucinations, although before, under the same conditions, it most emphatically insisted on their reality.*

The subject was put into hypnosis and a post-hypnotic suggestion was given to him that he would see his wife and child. When awake he began to smile, and when asked why he smiled he said: 'I see my wife and child'; but he wrote: 'I see nobody.' When put again in hypnosis he still continued to smile and said: 'I see my wife and child'; but he wrote (in hypnotic state): 'I do not see them really; I see nothing; I see my child, but I really see nothing.' "What do you mean," I asked, "by 'I see my child, but I really see nothing'?" To which he replied: 'I mean that I see my child in my mind only, but *'in honest'* I don't see anything.'

I then gave him a posthypnotic suggestion to see a snake. He claimed on awaking that he saw a snake. He manifested little fear. He certainly did not behave as if he really saw a snake and instead wrote "I see a snake. I see it in my mind." A great number of similar experiments were carried out by me varying the suggestions and all with the same results. I shall not burden the reader with a detailed account as they all gave identical results.

At first the automatic writing claimed emphatically the presence of the hallucinatory object and when the truth of the automatic writing was insisted on, the writing disclaimed fully the perception of the hallucinatory object. Finally we came on the real character of the suggested hallucination; 'I see my child but *honestly*, I do not see anything; I see my child in my mind only; I don't see anything.' In other words, if we take the facts plainly and do not play hide and seek with the subconscious, we come to the conclusion that in the suggested hallucinations the subject does not perceive anything as is the case in an actual hallucination but that he simply represents, vividly perhaps, what is suggested to him; in short he does not perceive but he simply *thinks* of the suggested hallucinatory object.

Another interesting point may be brought out here. The automatic writing, as is usually the case, is not taken cognizance of by the patient, and, although this same writing at first claimed the actuality and genuineness of the suggested hallucinations it gave in the second series of experiments the real insight into the whole matter: 'I see the child; I see nothing; I see it in my mind'; or 'I see the snake; I really see nothing; I see it in my mind.' When the subject was made fully conscious of his automatic writing and became fully aware that he was being entrapped he once more began to claim in automatic writing the actuality of the suggested hallucination. As long then as the automatic writing was regarded by the subject as independent for which he was not responsible and as long as the suggestion of the hallucination was not taken as directly addressed to it, the subject himself frankly acknowledged the fact that he did not see anything. The cat was thus let out of the bag. No sooner, however, was this truth of the automatic writing brought home to the subject so that he should be confronted with it directly and squarely, than he was bound by the fact of the given suggestion to claim that he actually saw the suggested hallucinatory object, although he really did not see anything at all. This is most instructive. For it shows clearly that the hypnotic consciousness, from the very nature of its heightened suggestibility, clings most anxiously to the given suggestion and insists on the reality of its fulfillment. We must, there-

fore, be on our guard and not trust the subject's introspective account, unless it is well sifted by good circumstantial evidence. It is because such precautions have not been taken in the close interrogation of the subject's actual state of mind, and because of the deep-rooted psychological fallacy as to the relation of ideational and perceptual activities that the prevalent belief in the validity of suggested hallucinations has passed unchallenged. If not for those factors, it seems to me, it would have been quite evident that *hypnotic and post-hypnotic suggested hallucinations are not genuine, but are essentially spurious; that hypnotic hallucinations, unlike actual hallucinations, are really not experienced; that hypnotically suggested hallucinations are only forms of delusions.*<sup>1</sup>

<sup>1</sup> The MS. of this article was received March 11, 1906. — ED.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

COMMUNICATED BY PROFESSOR JAMES R. ANGELL.

A STUDY OF CERTAIN RELATIONS OF ACCOMMODATION AND CONVERGENCE TO THE JUDGMENT OF THE THIRD DIMENSION.

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WITH THE COÖPERATION OF  
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I. While one of the writers of this paper was investigating the problem of the third dimension, controlling the depth location of combined visual images by obtaining a reflex control over the convergent-accommodatory process, the other who was serving as subject insisted that she possessed the ability to locate the image at will, irrespective of the reflex control exercised over the convergence by the conditions of the experiment. Furthermore, it was stated that she had possessed this peculiar ability of voluntary control of depth location throughout life and had often amused herself during youth by voluntarily changing the apparent distances of objects in the visual field. For instance, a house could be made to approach or recede at will. After becoming a psychologist her interests had led her along other lines, and the phenomenon had never been studied.

The results of the following tests show that this subject possesses a direct voluntary control of lenticular accommodation independent of the convergence, and that depth is in her case a function of accommodation and is in no respect *effectively* influenced by convergent changes.

In the experiments the subject's head was held by a mouth-bit head-rest. At the opposite end of the room (290 cm. distant) was a screen. Two bright wires of identical size and appearance were fixed parallel to one another in a screw mechanism

similar to the Jastrow æsthesiometer so that their distance apart could be gradually changed. The mechanism was fastened before the subject so that the wires were vertical and but a few inches in front of the eyes. A screen was interposed so that but a couple of inches of the tops of the two wires could be seen. The subject then combined stereoscopically the foveal images of these two wires and voluntarily controlled the depth location of the combined image. Under these conditions there is a reflexive tendency to maintain this binocular unity of vision and hence the experimenter can alter the degree of convergence as desired by gradually changing the distance between the two wires.

1. The experimenter kept the subject's eyes in a fixed convergent position, while the subject voluntarily moved the combined image back and forth between her head and the distant screen. This change in the depth location of the image was not a matter of mere interpretation but was an actually perceived movement. One's judgment as to the distance of an object may vary from time to time without any apparent motion on the part of that object. The distance may appear to be three feet at one time and eight feet at another. The change would be in the interpretative processes, in the motor evaluation of the constant visual criteria of depth. In our case, however, the combined image did not simply appear to be now nearer, or now farther away, but it also appeared to *move* as well. This apparent backward and forward movement was as real, psychically speaking, as that of any material object.

The following facts prove that the convergence really remained unaltered during this volitional movement of the image: (a) the combined images of the wires did not become doubled. Since the distance apart of the wires and their relation to the head were constant during the test, any convergent or divergent movements of the eyes would have destroyed unitary vision. (b) An object placed at the intersection of the visual axes was always seen single while objects placed nearer or farther away were seen double. (c) No convergent movement of the eyes could be detected by observation. If an observer sights over a fixed point at the juncture of the iris and the sclerotic the least

convergent rotation is easily detected. (*d*) The retina was also observed through an ophthalmoscope. A system of parallel vertical lines stretched on a frame was attached to the instrument in such a way that the retina was viewed through this non-magnified system of lines. The instrument was so adjusted that some prominent and distinct feature of the retina, *e. g.*, the edge of the optic disc or the juncture of two distinct blood vessels was in line with one of the vertical threads. Since the instrument magnified twelve to fourteen times, the least convergent rotation could be easily detected. Save a few irregular twitchings, no convergent motion occurred. A slight vertical rotation was present, a phenomenon which will be discussed later in the paper.

The following facts demonstrate the existence of accommodatory changes running parallel with the movement of the combined image. (*a*) The combined image became larger, more blurred and confused in proportion as its distance from the subject increased, and smaller, more distinct and definite in proportion as its location was changed to the distance position of the two wires. (*b*) The double images of any object placed at the position temporarily occupied by the movable image were always clear and distinct as in the case of a normal accommodation for this point. The images of an object placed at any other position were large, blurred and confused as in the case of imperfect accommodation. (*c*) In normal distance adjustments the size of the pupil varies reflexly with the lenticular changes. The same relation obtained in these tests. The pupil could be observed to enlarge when the image was moved to far distances and to decrease when the image was brought toward the subject. This change in the size of the pupil was also detected introspectively by the subject. In fact the experimenter's attention was first directed to the phenomenon in this way. (*d*) Lenticular changes were detected directly by the use of the phakoscope. The regulation Helmholtzian arrangement cannot be used in this test because the instrument would intercept the subject's binocular vision. A Welsbach gas light was covered so that no light was emitted in the room save through two small apertures. This light was directed upon the cornea. A candle screened from



the subject illumined the distant screen and a second candle was placed back and to one side of the subject so as to illumine the two wires without casting any light on the eye to be observed. In this way the room was sufficiently lighted to allow of a control of the movable image without interfering with the experimenter's vision of the reflected lenticular images. Since the eye was stationary, the lenticular images were continuously visible to the observer from one position. In fact, this is additional proof that the subject's eyes did not rotate, for the least convergent rotation would necessitate a new position of observation. The anterior lens image behaved exactly as in normal accommodation. It moved forward toward the cornea as the subject located the movable image at near distances and moved backward when the movable image was located at the screen. In certain of the tests the subject rapidly moved the combined image back and forth between her face and the screen, calling off the direction of the movements, while the observer simultaneously noted the behavior of the lenticular image. The observer also called off the movements of the lenticular image and had the results checked by the subject. With a little practice the experimenter could easily describe the behavior of the moving image of the wires by observing the accommodatory changes.

During the previous experiments the subject's eyes were kept in the one convergent position. The same tests were now repeated for several other convergent positions, varying from that of approximate parallelism to one of near convergence. The results were uniform and decisive throughout.

2. In the following series of tests the subject was requested to hold the combined image in some one position, *e. g.*, at the distant screen, while the experimenter altered the convergence as desired by means of the screw mechanism formerly described. The experiment was successful; the combined image was kept stationary at any desired position; the accommodation remained unaltered while the convergence was varied between normal limits.

The same line of evidence as used formerly proved the existence of convergent changes. The combined image remained single unless the eyes were forced beyond their normal conver-

gent limits or unless the enforced movements were too rapid or irregular. In this case the combined image would become double, the separation at once being noticed by the subject. Objects located at the point of intersection of the visual axes were seen single, while all objects outside of the temporary horopter appeared double. The convergent movement was easily observable.

The accommodation remained unaltered, for no visual images changed in size or distinctness. Objects situated at the location of the combined image appeared clear and distinct, all others being large and confused. No pupillary changes were detected by the observer. For the phakoscopic test it is necessary to keep the eye observed stationary. This can be done by moving but one of the wires in the mechanism. No accommodatory changes were observable.

The tests were repeated for different degrees of accommodation. The combined image was located at several intervening positions between the distant screen and a point 20 cm. in front of the subject's head. Uniform results were obtained in every case; the enforced convergent changes in no way effectively influenced the depth location of the combined image.

The subject was not immediately conscious of these enforced convergent rotations. At first the subject was directed to keep the combined image continuously at the screen, but was given no knowledge as to the test to be performed. The experimenter then changed the convergence back and forth between wide limits and questioned the subject as to what had occurred. The subject had no knowledge of these eye movements and expressed surprise upon being informed as to what had been done. This ignorance was no doubt partly due to the subject's extreme concentration of attention, for thereafter she was aware of these convergent rotations, though the awareness seemed to be based upon the behavior of the images in the visual field rather than upon an immediate consciousness of the eye movements themselves, *i. e.*, upon the afferent tactual-motor sensations.

This volitional control of the depth location of visual images is not limited to the conditions of the experiment, *i. e.*, to stere-

oscopically combined images. It extends to all normal conditions of vision. In the normal perception of any object, the subject can move its image nearer or farther away at will. Again no convergent movements of the eyes occur, for the moving image of the object remains single. Neither can convergent rotations be detected by observation of the cornea nor by ophthalmoscopic observation of the retina. The same relation obtains between the moving image and the accommodation as in the experiments above. The image is always located at that position for which the eyes are accommodated. These accompanying accommodatory changes are evidenced by the same tests described above.

This volitional control of depth obtains with monocular as well as binocular vision. The monocular control is probably conditioned by the same physiological changes as already described, though the proof of the statement is somewhat difficult. For instance, the experimental procedure used with the stereoscopically combined image cannot be employed, for binocular vision is necessary to secure such an image. If one eye be closed, it is impossible to make any confident assertion as to the presence or absence of convergent rotations. Observations can be made only under normal conditions when the vision of one eye is intercepted by a screen. Even here the conditions of the test are hardly fair, since binocular unity of vision is a stimulus to the maintenance of any given convergent position. The destruction of unitary vision by the interposition of the screen allows the eye observed to rotate slightly back toward its more normal position of rest, this rotation not being necessarily due to the voluntary alterations of the accommodation. However, as far as could be observed, convergent changes played no essential rôle in the monocular control of depth. The accommodation factor was present as formerly.

In the experiment on the combined image, a slight vertical rotation of the eyes was necessary in order to move the image as desired. An upward rotation was necessary to move the image away, with a downward rotation in order to bring the image nearer. This movement was first detected by the subject by noticing a slight shift in the position of the fixation point.

The observation was confirmed by the ophthalmoscopic test. The rotation was small but was further decreased in amount by practice. The angular rotation as measured by the ophthalmoscope was but one degree. However, this vertical rotation was necessary only in the case of the combined image. It did not occur for normal conditions of voluntary control, *i. e.*, for moving the image of any object normally perceived.

It is to be noted that in the experiment the combined image was moved up to and away from the distant screen. This seems rather anomalous, for if the depth location of visual images is a function solely of accommodation it would seem that all images in the visual field—the screen as well as the combined image—should move. Hence the movement of the combined image relative to that of the screen would be impossible. All images in the field should participate in the movement to the same extent. However, it is to be noted that the conditions of the experiment demand a strong concentration of the attention upon the combined image. Now it was found that the whole visual field,—or rather that part of it subject to accurate observation,—did participate in the third dimensional movements when the attention was voluntarily dispersed as widely as possible. In other words, the movements were limited to those images well within the focus of attention. With volitional control in normal perception, on the other hand, all images in the center of the visual field participate in the movements unless the attention is voluntarily concentrated upon some particular object. In that case this object alone will move although it is hardly proper to say that the remaining images appear stationary for the subject is hardly cognizant of them in any overt way. Consequently this volitional control of depth appears to be limited to those images within the field of attention. Depth is thus a function not merely of accommodation, but of the whole psycho-physical accommodatory act in which, however, convergence plays no essential or effective part.

With fatigue this volitional control over depth is weakened, or in other words, the subject loses control over the accommodatory process. In this condition the visual field tends to recede to a great distance and is only brought back to its normal posi-

tion by a distinct effort of will. At the same time the eyes tend to converge for near distances. The same results occur in the case of rest or complete relaxation. The eyes are then lowered slightly below the primary position and are converged upon a point some three or four feet distant. All images recede toward the horizon giving the subject a far off, isolated and detached feeling. From the pupillary observations as the subject falls into this half dreamy state of relaxation it is evident that the usual relation between accommodation and depth obtains. Hence there can be no fixed natural associative relation between the processes of convergence and accommodation in the case of this subject. In fact the natural relation between these processes would seem to be one of disassociation, the two becoming united or associated only in the *act of volition*. Again, in these conditions of disassociation, depth seems to be a function of the accommodatory process.

Outside of this peculiarity the subject's eyes are perfectly normal. The subject has never worn glasses and has been entirely free from eye troubles throughout life. In fact her vision has been exceptionally good. Several oculists have stated that her eyes are free from optical defects and are models in respect to general health and soundness.

The subject gives the following introspective account of the phenomenon: "From the earliest observation of my sense performances, I noticed that during eye fatigue the visual field would retreat to a great distance,—apparently to the horizon. In this condition the eyes felt relaxed and in a resting position. When the relaxation was less complete the field of view would be less remote, but still beyond its actual distance. This relaxation was subject to voluntary control and usually the amount of attention demanded by surrounding objects determined the degree of relaxation permitted. It was possible to move the field back and forth at will, but if the attention was strongly focussed on some central object that object alone appeared to move. Under natural conditions the whole field participates in the movement. It is becoming more and more difficult to keep the visual field in its proper position during fatigue, the constant tendency being for it to retreat to the horizon. In assum-

ing the position of rest, the eyes are directed downward, the visual axes being below the primary position.

"Under the experimental conditions, a slight upward and downward movement of the eyes was found necessary in order to secure the desired control. Under ordinary perceptual conditions this never occurs. When the visual object was kept stationary by voluntary maintenance of focus while the convergence was reflexly altered, no sensation of eye movement occurred. There was present merely a feeling of strain, or of constant tension *within* the eye. When the eyes are converged and focussed upon a near object, there is always present a slight sensation of strain *within* the eyes. Upon allowing the object to retreat to a far distance, the feeling is one of relaxation,—a relief of tension within the eyes. No sensations are present in the extrinsic eye muscles."

In regard to this subject the following facts seem noteworthy:

1. No fixed nativistic connection exists between accommodation and convergence. They are related only indirectly through the process of innervation.
2. There is complete volitional control exercised over the lens independent of the convergence.
3. Depth is a function of the psycho-physical accommodatory act and is not effectively influenced by reflexly induced convergent changes.
4. The subject seems to be peculiarly non-sensitive to convergent rotations, but is extremely sensitive to accommodatory changes.

II. Afterwards a somewhat similar case was discovered. The subject, Mrs. V. H., wife of a prominent surgeon of Chicago, is astigmatic and has experienced much trouble in securing accurately fitting glasses. This has resulted from the impossibility of completely paralyzing the lenticular muscles by atropine. A well known oculist of Chicago continued the treatment until compelled to stop for fear of causing a permanent injury, yet spasmodic twitchings were present sufficiently to prevent any accurate diagnosis. The subject's eyes are extremely susceptible to fatigue and in this condition all objects



appear far off and distant, it being extremely difficult to bring them back to their proper positions. The same is true of rest or relaxation. All visual objects at once recede to distant positions, but can be pulled back by an effort of will. The subject has found it almost impossible to estimate distances accurately because the apparent distances of objects vary markedly from time to time. The resulting conflict of judgments as to the same distance first brought this peculiarity to the subject's notice. No inconvenience has resulted in familiar surroundings because the varying visual distances are ignored in favor of the actual knowledge of the particular situation as reflectively derived from past experiences. However, with relatively strange surroundings she has learned to place no reliance whatsoever upon estimations of unknown distances. For instance, the subject related that she is a golf enthusiast but has always remained an extremely poor player on account of this visual trouble. When approaching a green on unknown grounds where the strength and kind of a stroke must depend upon the actual distance of the green, often to her amazement the ball would be sent flying far beyond, or occasionally it would fall far short of its intended destination. The fault lay not in a misjudgment as to the proper stroke adapted to any given distance, but rather in a *visual* misjudgment of the distance. On familiar grounds the same trouble occurred except when certain fixed objects were present whose space relations had become known from former experiences. Even here no high degree of accuracy could be obtained. The case is somewhat similar to the distance illusions in high altitudes. A mountain appears but a mile distant, yet a five-mile tramp is found necessary to reach it. In this case, however, all distances appear shortened, but in a certain definite ratio, and consequently after a short time the novice learns to make a definite allowance for the illusion. In other words, the illusion is constant in character and a new set of motor interpretations of visual distances is acquired. With the present subject the illusion is variable. The magnification or decrease of actual distances varies from day to day and hence from a few experiences one cannot derive any fixed system of motor interpretation of apparent, or visual, distances that will apply

accurately to novel situations. Adaptive evaluations of apparent distances cannot be successfully universalized. Every specific distance between two objects must be learned mainly by itself. The situation is so novel and foreign to the ordinary experience that one finds it rather difficult even to imagine, to say nothing of finding words adequately to describe, the phenomenon.

Circumstances did not permit any detailed tests upon the accompanying physiological changes. The subject declared that the fixated object never became doubled when its image retreated or approached. Tests demonstrated that she readily detected double images when present. A close observation of her eyes convinced the writer that no convergent movements occurred during the movements of the visual field. As to accommodatory changes the size of the pupil varied as in the case of the first subject. The distinctness and clearness of the movable images also varied, thus indicating lenticular disturbances. On the basis of the similarity between the two cases, the evidence seems sufficient to warrant the conclusions that this subject also possessed a volitional control over accommodation irrespective of the convergence and that depth is a function of the psychophysical process of accommodation and is not effectively influenced by convergent changes.

In a way it is hardly proper to speak of this second case as one of voluntary control of depth; it is rather to be described as a lack of perfect control. Depth is subject to volition but the control is difficult, erratic and inaccurate.

III. These two cases have a significant bearing upon two questions: (1) the mutual relation of the two physiological processes of convergence and accommodation from the standpoint of their volitional manipulation; (2) their relative influence upon judgments of depth. Their bearing upon both questions can best be interpreted in the light of other similar results.

1. The mere fact of the disassociation of the two processes is not new. The following statements in regard to the phenomenon are probably rather generally admitted: In normal perceptual conditions a relatively strict association of the two functions obtains. The two processes *can* be disassociated to some degree, at least with most people. The degree of the disasso-

ciation and the facility of its attainment vary markedly among individuals and probably with practice. For instance Hyslop<sup>1</sup> and the two subjects described in this paper represent the extreme as to disassociative ability. Most people can obtain but a limited degree of disassociation, and that only with difficulty.

Now there are at least two methods of obtaining disassociation, that typified by Hyslop and that represented by the two subjects of this paper. Hyslop has written rather fully of his case which is characterized by a very unusual control of convergence. He voluntarily converges in front of or behind a double figure combining the similar images, but the lens fails to follow this convergent change and remains accommodated for the actual position of the object in question. This method is the exact opposite of the second type. In both cases the volition is directed away from the object. In the first type the convergence responds to the volitional innervation while the lens remains unaltered; in the second type it is the lens which moves in conformity to the will while it is the convergence which remains adapted to the position of the object. In normal distance adjustments the two processes would alter in consonance for both cases.

This process of disassociation may be conceived of in either of two ways: (1) there is a normal associative force or relation — whatever its nature may be — between accommodation and convergence, which is volitionally destroyed for the time being, leaving one of the processes free from any stimulating influences; (2) the process which remains unaltered during the disassociation is under the influence of two antagonistic forces, the normal associative relation and some objective stimulus from the object.

The latter conception is supported by several lines of evidence. In the first, or normal, type of dissociation, the lens at first alters in consonance with the convergence with most people and after some time gradually becomes re-accommodated for the position of the object. According to the first conception no re-accommodation should occur. With Miss Allen, the con-

<sup>1</sup> Hyslop, 'Experiments in Space Perception,' *PSYCH. REVIEW*, I., 257-273, 581-601.

vergence changes as the relation of the object — the two parallel wires — is altered in respect to the eyes. The normal connection does not exist during fatigue nor relaxation.

This tendency to respond to the object is a volitional-reflexive one. It cannot be purely reflexive, for the response is not to *any* object in the visual field, but only to that object whose image occupies the focus of attention. On the other hand, it cannot be purely voluntary, for in the experiment the distance apart of the two wires was changed and the subject was at first entirely unconscious of the induced convergent changes. Given attention to a combined or nearly combined image, the convergence is reflexly altered so as to maintain or secure respectively binocular unity of vision. Given attention to a confused image, the lens is reflexly stimulated so as to secure clear and distinct vision. The response is thus reflexive in nature, but operates under volitionally imposed conditions; it is only indirectly subject to volition.

Since this process, which remains unaltered in disassociation, is subject to two antagonistic forces, the results — the degree of disassociation and the ease and rapidity of its attainment — must depend upon the relative strength of these forces in any particular situation. Since the results vary markedly with individuals and to some extent with practice, it follows that the relative strength of the two forces is a wide variant. The responsive tendency to the objective stimulus is basically reflexive and it is of the nature of these nativistic reflexive tendencies to be racially uniform. Consequently it must be the strength of the normal associative tendency which varies among individuals and with practice. In fact, this assumption is supported by another line of evidence.

This normal associative relation between convergence and accommodation may be conceived of in either of two ways: (1) It may be nativistically functional in nature. One process may be volitionally controlled while the second process has a reflex relation to the first. That is, the actual movement due to volition is the physiological stimulus to the second process, as in the case of a chain of reflexes. This supposition would demand two totally different sets of anatomical conditions to

explain the two types of disassociation, but anatomy is hardly supposed to be subject to such marked individual variations. Again the two processes are disassociated with Miss Allen in the condition of rest or relaxation. Here the association obtains only under the conditions of innervation. The associative tendency cannot be a natural nativistic one. The two processes are related not directly but only indirectly by means of their common relation to innervation. (2) The second hypothesis conceives the two factors as connected only in the process of innervation, and in such a way that individual variations may arise during the development of volitional control. Two possibilities are again open: (a) The two processes were originally distinct and separate volitional acts, which in time were combined, or coördinated into one act, as is the case with many of our complicated habits. The method of development is one of synthesis. The connection is thus not nativistic, but acquired and subject to individual variation. (b) The second possibility differs from the first in the fact that the growth of motor control proceeds by analysis and differentiation rather than by synthesis and integration. As an illustration, a baby attempts a definite movement of its big toe, but as a result the other toes and the foot as a whole receive a definite innervation. In time this vague whole may be differentiated into separate volitional acts, or into certain coördinations wherein the parts bear certain mutual relations to each other. In other words, individual variations may arise during the development of motor control.

From the standpoint of this paper it makes no difference which of these possibilities we assume to be true in the case of accommodation. Nor have any facts been developed which definitely point to either conclusion. However, we know that in general it is those movements whose anatomical conditions of innervation are widely distinct and separate, which are synthetically combined while those members closely related anatomically, *e. g.*, the toes of one foot, are originally united in volition and become differentiated in the development of volitional control. This general principle would support the second hypothesis as to the relation of convergence and accommodation.

According to both conceptions, however, it is possible that

either convergence or accommodation might play the dominant rôle in volitionally controlling the coördination. In a coördination of two factors, 'a' and 'b,' it might be possible to innervate 'a' separately, while on the other hand the innervation of 'b' would always involve that of 'a.' For instance, most people can move the big toe without innervating the little toe any great amount, while the movement of the latter generally involves a strong innervation of the former. Thus we could have two extreme types of volitional control in one of which convergence would possess the maximum of strength or importance, while the accommodation would play the dominant rôle in the other. In disassociation, the dominant process would be subject to volition, while the subordinate, or weakly innervated, factor would come under the influence of the antagonistic tendency to respond to the objective stimulus. The strength of the normal connection between the two processes would depend upon the relative strength of their innervation, an individual variant and subject to practice and development. Hence the theory would account for the facts of disassociation, — the two methods of obtaining it, the varying degrees of disassociative ability, and the probable effects of practice. One point to be noted in this theory is that the two types of disassociation also mean two types of voluntary control over distance adjustments; convergence has the maximum of functional importance in one case, and accommodation in the other. Moreover, the theory will allow of other degrees of relative importance intervening between these two extremes.

It is idle to speculate on all the possible causes determining the development of these types, for they are too numerous. One deserves to be mentioned, however. The afferent stimulating results of each process might differ with individuals. One person might be very sensitive to convergent changes and relatively insensitive to changes in the accommodatory mechanism. This greater afferent stimulus might react so as to emphasize the corresponding innervating factor. Whether this conception is probable or not we do not pretend to say. However, it is to be noted that most people are the more sensitive to convergent changes and belong to the type of voluntary control



represented by Hyslop. On the other hand, Miss Allen is extremely sensitive to accommodatory changes and relatively insensitive to convergent movements, while accommodation possesses the maximum of volitional importance in her case.

2. The second main question deals with the relative influence of accommodation and convergence upon depth. In Hyslop's case of disassociation the distance of the combined image depended upon the convergence, the image always being located at the point of intersection of the visual axes. This result is the exact opposite of that obtained from the two subjects of this paper. However, the results agree in one respect, — depth is a function of that process possessing the most volitional importance and is not effectively influenced by the second, or volitionally subordinate process. This statement does not necessarily mean that the less important process in no way *conditions* depth, for it is entirely possible that a process may be one of the necessary conditioning factors of a result and yet in certain situations its independent variation may cause no appreciable change in that result. Especially is this true of methods of isolation.

This general problem has been a very prominent one in experimental psychology. Knowing that depth is a function of the convergent-accommodatory act, many experimenters<sup>1</sup> have attempted to evaluate the relative importance of each factor by some method of isolation. It is not our purpose to discuss and criticize these papers. We merely wish to point out the fact that their results do not altogether agree.

Since the tacit assumption seems frequently to have been made that some definite and normal relationship must exist applicable to all methods of procedure and to all individuals, these diverse results have led to an almost hopeless contro-

<sup>1</sup> Arrer, 'Ueber die Bedeutung der Convergenz- und Accommodationsbewegungen für die Tiefenwahrnehmung,' *Phil. Studien.*, XIII., 116-161, 222-304. Baird, 'Influence of Accommodation and Convergence upon the Perception of Depth,' *Amer. Jour. Psychol.*, XIV., 150-200 (admirable résumé of important literature). Dixon, 'On the Relation of Accommodation and Convergence to our Sense of Depth,' *Mind*, N. S., IV., 195-212. Hillebrand, 'Das Verhältnis von Accommodation und Konvergenz zur Tiefenlokalisation,' *Zeitsch. für Psych.*, VII., 97-151. Also on the same subject, *ibid.*, XVI., 71-151. Wundt, 'Zur Theorie der visuellen Raumwahrnehmung,' *Phil. Stud.*, XIV., 1-118. Rivers, 'The Apparent Size of Objects,' *Mind*, N. S., V., 71-80. Discusses reactions caused by drugs affecting the accommodatory mechanism.

versy. The conflict is a result of the assumption. Now this assumption can be defended upon no *a priori* grounds. It is based merely upon our normal desire for, and habitual expectancy of, uniformity. May the truth not be, as the results indicate, that the relative efficiency of the two processes in determining depth is an individual variant and hence no uniform results are to be expected? The problem may belong to the domain of individual and not general psychology.<sup>1</sup> Since there are no valid reasons for not doing so, let us for the time being accept these diverse results at their face value.

3. Then we should have the following facts: With two subjects depth is a function of accommodation; in another case depth depends upon the convergence and in another set of cases, the relative efficiency of the two factors varies among individuals. These results can be summarized in the general proposition that the relative influence of accommodation and convergence upon the sense of depth is a wide individual variant, possibly representing all the intervening stages between the two extremes.

In regard to the first problem as to the relative volitional importance of each process in distance adjustments we have a similar proposition: The relative volitional importance of the two processes, or the relative strength of their innervation, varies among individuals between the two extremes.

Not only are the two propositions similar, but we find between the two series a point of connection for the extremes. In those cases depth is a function of that process possessing the greater volitional importance. If we infer that what is true for the extremes is true for the intervening cases, we have the general theory that depth is a function of each process in proportion to its volitional importance, or in proportion to the strength of its innervating impulse in any particular situation.

This position finds some support from another set of facts. In a recent article<sup>1</sup> one of the writers maintained the thesis that depth is a function of the innervating impulse leading to the convergent-accommodatory act. It was denied that the con-

<sup>1</sup> Cf. Hyslop, 'Psychic Synthesis in Vision,' *Mind*, XIII., pp. 499-526; 'Some Facts of Binocular Vision,' *Mind*, XIV., 393-401. Baird, *loc. cit.*

ception shed any light upon, or involved in any way, the problem of the mutual relative importance of the two associated processes. This reservation was explicitly made in consideration of the experimental results developed in the present paper. The point to be noted here is the general agreement of that theory developed from an entirely diverse set of facts with the present conception, *viz.*, the dependence of depth in some way upon the innervating, or volitional aspect, of distance adjustments.<sup>2</sup> If depth be a function of the impulse innervating the whole adaptive mechanism, it is by no means an illogical inference to suppose that the innervating elements exercise a relative functional efficiency upon depth in proportion to their relative strength, — the theory advanced in this paper.

According to this conception of the normal associative relation between the two processes, it would follow that their relative influence upon depth would vary not only among individuals but also in the same person during the development of ocular control, with practice, and possibly according to the experimental procedure. The theory not only has a basis in fact but possesses a functional importance for a further study of this intricate question from a genetic standpoint.<sup>3</sup>

<sup>1</sup> Carr, 'A Visual Illusion of Motion During Eye Closure,' PSYCH. REVIEW — MONOGRAPH SUPPLEMENT, 1906.

<sup>2</sup> Cf. for a recent statement of certain facts bearing on the motor-sensation theories of space perception, Judd, PSYCH. REVIEW, IV., 374-389.

<sup>3</sup> Just as the above article was going to press, the writer received a note from Professor S. S. Colvin, of the University of Illinois, regarding a case which has come under his observation. A friend of his was once afflicted with cataract in both eyes, and during this period experienced from time to time the phenomenon of objects receding to a great distance. Since the performance of an operation, this visual peculiarity has entirely disappeared. With the information given, the case is subject to various interpretations, but it is certainly suggestive from the standpoint of this article. Through the courtesy of Professor Colvin, more definite and detailed information is expected. Since the completion of the MS. several new cases have come to the writer's knowledge, presenting some new details though supporting the main contention of the article. Some facts indicate that possibly the phenomenon is of more frequent occurrence during youth and often disappears with maturity. If this be true, the subject possesses interest for further observations from the genetic standpoint.

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## ILLUSIONS OF REVERSIBLE PERSPECTIVE.

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This is a sketch of a work concerning the optical inversion that is observable in geometric figures and perspective designs. The special characteristic of these figures is that they call into consciousness two or more different interpretations. Thus for instance, if one fixes the point of intersection of two straight lines of a hexahedron he perceives it with one of its surfaces either inclined downward or lifted up, by a corresponding change of the position of the axis.

The illusory phenomena observed in a great many such figures have been explained by three different kinds of theories, namely by psychological, physiological, and psycho-physical theories.

I. Psychological explanations of the phenomenon are mostly generalizing conceptions, according to which inversion is a product of will, imagination, false judgment, or attention. Weadstone,<sup>1</sup> for instance, in his critique of the physiological theory of Necker, maintains that inversion is an effect of intuition. Hering,<sup>2</sup> in his 'Physiological Optics' says that the question of what kind of inversion will appear is largely a matter of chance or volition. Helmholtz asserts the same thing. He says that the perception of perspective figures has its cause in imagination and especially in the images of memory.<sup>3</sup> If I imagine vividly one or the other form of the figure its apparition is imminent. Or "we can also produce intentionally a change of inversion, if we recall vividly the image of its contrary form. When its resemblance to that of the figure looked at is established, then the perspective remains stationary without effort." Helmholtz

<sup>1</sup> *Poggendorfs Annalen*, I., 51.

<sup>2</sup> *Hermann, Handbuch der Physiologie*, 580.

<sup>3</sup> *Physiologische Optik*, 771-777.

insists that: "Glancing at a figure we observe spontaneously one or the other form of perspective and usually the one that is associated in our memory with the greatest number of images." But Helmholtz and Hering have called our attention to many physiological details characterizing inversion. Hering for instance notes that changes of light and shade necessarily tend to produce such illusion, and he mentions the acceleration of changes by practice. Helmholtz notes the influence of passiveness of the eye in the production of inversion.

II. Two different tendencies are to be noticed in physiological theories: the first regards perspective figures as produced by changes of accommodation, the other regards them as produced strictly by movement of the eye. Necker<sup>1</sup> after having denied that inversion could be an effect of will or accident selected accommodation as its principal cause.

By this he understands the relation existing between different points of the observed object and the central and peripheral parts of the retina. The part of the figure that is perceived with the fovea is estimated as nearer to the eye, the point observed by the peripheral parts of the retina as more remote.

Plausible as this theory appears, it does not take into account one of the principal phenomena of inversion, the absence of this illusion in certain cases of binocular and monocular vision. Secondly, Necker's theory is not to be considered as an explanation of the phenomena of accommodation, for we know that the same relations between the observed object and the special parts of the retina exist quite apart from accommodation.

Many years afterwards Loeb explained inversion by the same theory of accommodation, but he used it in its real significance, namely as innervation of the eye. His theory touches unquestionably one of the most important conditions of inversion, but it does not explain all the associated phenomena. For instance Loeb says that we obtain the impression of concavity in a perspective figure if the innervation removes the point of fixation, or that we have the impression of convexity by bringing the eye nearer to the figure. An easy experiment shows

<sup>1</sup> *Annal. der Physik u. Chemie.*

<sup>2</sup> *Pfluegers Archiv*, 1886, 411, 274.

however that once we perceive a durable inversion we can change the accommodation successively and the image of inversion stands still. It is often necessary to open the second eye in order to escape the persistent illusion.

The most recent of physiological theories is that of W. Wundt recorded in his work on geometric-optical illusions. Inversion according to Wundt<sup>1</sup> is caused by determined movement, consequently by change of points and lines of fixation. Wundt says: "The image of the retina ought to have a determined position if a perspective illusion is to appear; but the form of this illusion is entirely dependent on motion and direction of vision." Wundt indicates exactly what points and what lines ought to be fixed in order to obtain a concave or a convex form of inversion. Attentive observation proves, however, that if these movements can actually facilitate inversion they are not absolutely necessary to its appearance. It escapes the attention of Wundt that we can observe different forms of inversion as well if we fix only one and the same point or the same line of the figure.

We have yet to mention the experiments of Professor Mach,<sup>2</sup> of Vienna, which were applied chiefly to solid objects. They constitute the largest contribution yet made to our knowledge of the sum of the phenomena of inversion. Thus he observes the inversion produced by change of intensity in light and shade, or by different inclinations of the object, and he describes the phenomena of movement which accompany inversion. But Mach having described all the details does not explain the cause producing them.

III. As for the special psycho-physical theories, which make use of the known facts in the functioning of the nervous system, they are mostly connected with more or less ingenious psychological hypotheses that have contributed little to the solution of the question. Such is for instance the theory of N. Lange, which explains inversion with the help of attention and of apperception. Lange<sup>3</sup> has otherwise the merit of first trying to measure the rapidity with which the inversion occurs.

<sup>1</sup> *Die geometrisch-optischen Täuschungen.*

<sup>2</sup> *Beiträge zur Analyse der Empfindungen*, 86, and *Wiener Sitzungsberichte Math.-naturwiss. Classe*, 54.

<sup>3</sup> *Phil. Stud.*, Band IV., 405, 1887.



Because of the lack of a definite solution of the problem of inversion, a series of new experiments seemed necessary.

1. The first object of these experiments was to ascertain the degree of rapidity with which we perceive inversion in plane figures. Internal observation has already permitted us to suppose this rapidity greater than the rate of respiration, but slower than that of the pulse. Consequently the investigation was made in such a way as to observe the relation between the rapidity of inversion and the frequency of the pulse.

The result that was obtained by the graphic method, while the experimenter regarded intently a plane figure (Fig. 1 made of copper wire), proved on the one hand that synchronism between

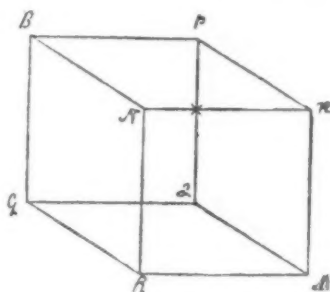


FIG. 1.

TABLE I. (Observer A).

	Pulse.	Inversion.		Pulse.	Inversion.		Pulse.	Inversion.		Pulse.	Inversion.	
30/X	21	14	4/XI	7	5	9/XI	10	10	12/XI	16	20	
	39	27		37	23		51	52		17	21	
	8	6		17	13		28	27		21	26	
	17	12		29	20		31	30		36	47	
	9	8		47	46		9	10				
	38	28		35	33		38	39				
	9	6		58	65		17	18				
	7	5		15	16		15	14				
	17	12		23	21		56	60				
	9	6		40	38		42	47				
	7	5		12	12		16	18				
	37	23		20	29		30	40				

TABLE II. (Observer B).

	Pulse.	Inversion.	Pulse.	Inversion.	Pulse.	Inversion.	Pulse.	Inversion.		
30/X	7	7	10/XI	10	10	8	8	5	5	
	4	4		10	10	5	5	10	10	
	5	5		6	6	17	17	15	14	
	7	7		4	4	8	8	4	4	
	8	8		17	8	5	5	3	3	
	10	10		5	3	8	8	20/XI	45	46
	9	9		18	10	4	4	28	28	
	8	8		5	3	3	3	17	13	
	9	9		10	7	5	5	7	7	
	7	7		12	12	6	6	38	28	

the two does not exist, the rapidity of inversion being often inferior or superior to the rapidity of the pulse. It confirmed, on the other hand, what was already proved by internal observation, that these two rates of rapidity are very nearly the same.

In one person 124 inversions occurred in 174 pulsations; in another 260 inversions in 265 pulsations, and 313 inversions in 362 pulsations.

2. The fact that all persons engaged in these experiments declared positively that they had the feeling of an inward movement of the eye, even during an intense fixation of the indicated point of the figure, led to a question of a different kind, namely, what sort of movement it could be. With the help of an

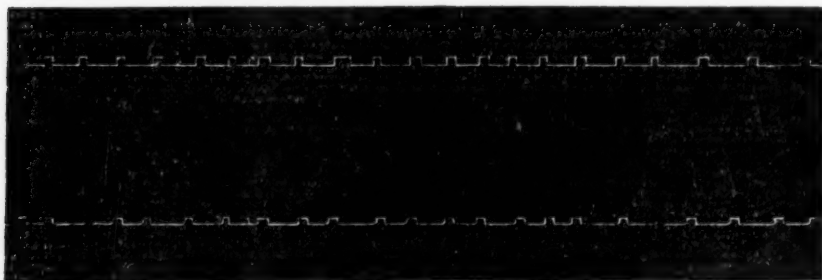


FIG. 2.

ophthalmometer the eye of a myopic person (7 dyoptrics) was observed. The observations of light images of the external lens proved that every inversion produced in a plane figure is accompanied by a change of refraction. These changes are so distinct that it was not difficult at all to mark them graphically, when the observed person marked at the same time the changes of inversion, having fixed monocularly the hexahedron made of copper wire (Fig. 2). Of 543 double reactions obtained in this way, 343 were simultaneous notations of the changes of luminous images observed by the experimenter in the ophthalmometer and the notations of inversions by the observer of the hexahedron. In 132 cases this synchronism was dubious; in 68 the reaction failed completely, a fact which is explained by a decrease of attention on the part of the experimenter, or by a disturbing movement of the strongly lighted eye of the observed

person. No series of these experiments lasted longer than a minute. These numbers proved distinctly the fact that the inversion of the plane figures can not be realized without a notable change of refraction.

3. But while the two preceding series of experiments were conducted on plane figures, it was necessary to complete them with experiments made on figures of three dimensions. To this end were prepared many figures of copper wire and many solid objects of small size.

But few trials were needed to show that the inversion appears as well in the case of solid objects as in that of plane figures. Almost any object can be used to illustrate the inversion ; for instance, a chair, a round or oval table, a lampshade, an umbrella — anything.

During these observations four main phenomena have been found to be characteristic of the inversions : (*a*) changes in the intensity of light, in tone of color, and in the form of the observed object, (*b*) the duration of the illusory image, (*c*) the illusory movement, (*d*) the inclination of the illusory image. But before we enter into the description of special phenomena of inversion a few words should be said concerning the difference of character between the inversion of plane figures and that of solid. Likewise something should be said about the difference in physiological conditions that go to produce inversions.

The difference between the plane and the solid figures consists in the fact that the first gives the sensation of two or more illusory forms, while the other presents but one. For instance : if we gaze intensely at the point of section of two lines of Necker's hexahedron, this plane figure appears immediately in three dimensions, one of its surfaces being inclined downward. This is the first illusory form. If we continue to fixate the same point of the figure, we perceive that this first illusory form has been transformed in such a way that we now see the same hexahedron lifted up. This is the second illusory form. Thus this inversion consists : (1) Of a passage of the normal sensation of a plane figure to the sensation of the first illusory form, (2) of the passage of the first to the second illusory form. But if instead of a perspective design we used a figure of three dimen-

sions (of copper wire), then, having fixed the same point, we obtain only one form of illusion. Namely, if we observe an inclined surface of the figure we perceive the illusion as lifted up. If on the contrary we observe one that is elevated we have the illusion of an inclined figure. In both cases the illusion presents to the eye the passage of a real into an illusory sensation.

In the consideration of this difference of character in the two inversions we will distinguish, in what will follow, the inversion with one illusory form from the inversion with two such forms. The inversion which consists in the passage of a normal sensation to an illusory sensation will be called *real-illusory inversion*; the inversion which is the passage of one to the second illusory form will be called the *pure-illusory inversion*. This distinction is very important, because the physiological conditions necessary to produce both are not quite the same. To obtain a real-illusory form of inversion in any object or figure, cited above, it is sufficient to fix intensively and binocularly one point of the figure and presently to cover one eye with the hand in such a way that the eye remains in the greatest possible tranquillity. The inversion will not fail to appear in these conditions, and the degree of the passivity of the eye will decide the rapidity with which the inversion occurs. This effect can still further be facilitated by inhibition of all efforts to perceive the real dimensions of the object. The case is not the same in the inversion with double illusory form in plane figures. When the gaze is fixed in the same way they show the inversion marked by transition from a real to an illusory sensation, but they do not produce the passage of one to the other purely illusory inversion, unless there is either a movement of the ocular globe (Wundt's method) or a change of accommodation (Necker and Loeb) confirmed by the ophthalmometric observations of this essay. But by following either of these methods we can easily evoke both the forms of inversion above mentioned.

We now return to the description of the four kinds of phenomena, mentioned above, that characterize inversion.

(a) In the first place we have to do with the changes in the intensity of light, the shade of color, and the form of the observed object. All these details can easily be followed by using

a visiting card in the same way as it was used by Mach. If this card, being first bent in the middle, is fixated monocularly at a point of its concave surface, we perceive it immediately as an illusory convex card. The illuminated part of the figure is now much more brilliant, the shadow deeper than it was in the moment preceding the inversion, or, strictly speaking, in the moment preceding the closing of the eye.

The shadowed part changes at the same time in color, passing through various tones, from gray or brown to yellowish-green. This is especially noticeable if we change the position of the head without altering the point of fixation. Given the same conditions the form of the card is modified again.

(b) The real-illusory image acquires a certain duration, difficult to maintain in the plane figure, but very easy in the figure of three dimensions. So for instance the inversion of a concave cone, made of copper wire, does not disappear when observed monocularly, even or when the observer retires and re-approaches a distance of a few meters; or when the point of fixation is changed, or when the observer lays down or takes up his glasses, or even when the observer closes the eye for 2-5 seconds. As soon as the eye is opened the inversion reappears again.

(3) Illusory movement is the third characteristic phenomenon of inversion. It can be observed in both the plane and the solid figure. If an illusory image attains a certain duration in the consciousness, nothing is easier than to put it in an illusory motion. This result can be obtained by quietly changing the position of the head in any direction. The retinal image then changes exactly as it would change if the observed object were in movement. If we observe for instance the illusory image of a concave cone while the gaze is fixed on the central point of the figure, the illusory image will be like that in (a) Fig. 3. As soon however as the position of the head is directed to right or left, upward or downward, the image will appear in quite other perspective, such as (b) and (c).

Supposing now this movement is very slow and progressive. Then the sensation given by one point of view will join with that given by another point of view, etc.; we perceive the ob-

ject in a synthesis of successive phases, consequently in what seems continuous movement. But if the illusion is to be perfect, it is necessary to change the angle of vision carefully and

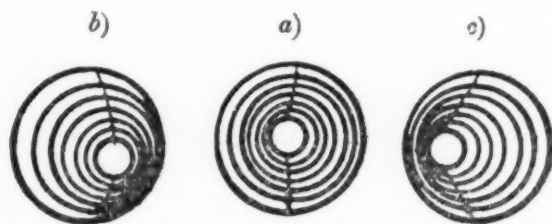


FIG. 3.

systematically, the gaze being strictly vis-à-vis to the observed figure. That is to say, the line that joins the fovea with the fixed point of the object must be the radius of a circle in which the head moves. The ciliary muscles should be entirely passive. (In order to facilitate this circular movement, a circle was made on the floor with chalk around the table which sustained the figure.) Under these conditions all the perspective phases of the illusory image display themselves freely, from the first front view to the extreme right or left view, where the illusory image attains the maximum of abbreviation, before it disappears completely. This passage from the maximum abbreviation of the image to complete disappearance is very characteristic, because once coming to this maximal point our attention finds itself in a moment of doubt, whether the observed image is an illusion or a reality. But having advanced a little in the same circular direction we are reassured on this point. We then feel a kind of start in the eye produced by the disappearance of inversion and particularly of the illusory movement and the apparition of the real object remaining in the same place as before.

(c) The illusory movement in the plane figures (of copper wire) can easily be observed if one acquires a certain facility in preserving a durable inversion. So, for instance (Fig. 4), if the observer places himself at point (a) (nearly  $60^\circ$  from the line  $b-b_1$ ) of the circular line that surrounds the Necker's plane figure of copper wire, he has to fix monocularly the point marked



with a small cross and then to cover the second eye with the hand. He will obtain immediately the illusory image, namely, the illusion of a lifted hexahedron.

Having succeeded in making this illusion durable we have to follow very slowly and quietly the circular way from  $a$  to  $b_1$ . The image of inversion passes during this time through a series of perspective phases, but, strange to say, these changes do not develop themselves in the same way throughout the arc.

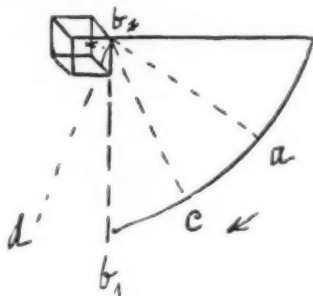


FIG. 4.

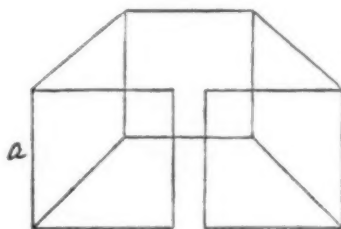


FIG. 5.

From  $a$  to  $c$  we observe the well-known changes of perspective phases, but from  $c$  to  $b_1$  the perspective phases disappear at once and we are startled with an image of the entire plane figure that displays a movement of rotation around its axis, which axis is formed by one of the lateral sides produced. Now the entire figure moves like a door on its hinge. This movement propagates itself without interruption if we continue the movement of the head forward and downward between  $c$  and  $b$ . During these observations the figure appears continually as displaced, and this displacement is determined by the point held by the eye. If the eye is for instance in the point  $c$   $30^\circ$  from the line  $b-b_1$  the figure takes the direction of  $d-b_1$  and so forth.

We can perceive the same illusion in many other figures, such as Fig. 5 (which must be fixated in the middle point of a lateral line, as  $a$ ).

(d) There remains now the description of a most important phenomenon of inversion, namely, the inclination of the illusory image. This inclination is again in direct connection with



changing the point of vision in the right or the left direction. These phenomena may be corroborated by observing a concave card instead of a convex one. Here as in the preceding case are to be noticed: (1) The point at which the inversion is nearly plane and that at which it disappears completely ( $x, x_1$ ); (2) The points of maximum and minimum inclination  $r, r_1, r_2$ .

These relations will be the same if instead of moving the head upon a circular line, we observe the object during its rotation around a horizontal axis. The direction however of the movement of the card will be changed: if we move it downward we see rotation in the opposite direction.

These experiments prove that the relation between the movement of the head and the inclinations of the illusory image is a fixed relation. The inclination of the illusory image is a function of the angle at which the eye observes the object.

Summarizing the results obtained from these experiments, we observe:

1. That the maximum of rapidity of inversion is nearly the same as the frequency of the pulse, though the relation is not functional.
2. Inversion in plane figures does not appear without a change of refraction.
3. We have to distinguish real-illusory inversion, and a pure-illusory inversion which involves different physiological conditions.
4. Inversion appears in plane as well as in solid figures, and is accompanied by four distinct phenomena: (*a*) changes in the intensity of light, in the tone of color and in the form of the observed object; (*b*) duration in the illusory image; (*c*) movement of the illusory image; (*d*) inclination, which is a function of the angle at which the eye observes the object. These observations as well as the corresponding experiments permit us to explain the phenomena of inversion in the following way.

It is certain that the cause of inversion consists in the relation between the observed object and the central and peripheral parts of the retina in which the image of the object produces itself. The points that fall upon the fovea are estimated more distinctly and consequently as nearer to the eye; those on the

contrary that fall upon the peripheral parts, being less distinct, are considered as more remote.

These relations however are not sufficient to explain all the phenomena of inversion (its absence, for instance, in many cases of binocular or monocular vision) unless we take account of a certain irregularity of the vision when this illusion occurs.

The perception of an exterior object in its three dimensions can generally be the effect of binocular or monocular vision. In the binocular vision, as we know, the image of the retina, of the left eye for instance, is completed and controlled by the image of the right retina. Accordingly the coöperation of these two images, and at the same time the influence of accommodation and especially of convergence produce a normal representation of an object in space.

It is the same in monocular vision. We get no impression of the depth of an object with only one image of the retina, but only by two or more successive images of the same retina that complete and control each other. The representation of the object therefore is a result of two retinal images produced by the rays of light coming from two opposite directions.

It is quite different in the case of the illusion that we speak of. If we fix the object after closing one eye and by trying to immobilize it as much as possible, we suppress at the same time the control necessary to perceive the dimensions of the object. Closing one eye, we suppress the simultaneous image of the second retina; making the eye immobile during the monocular vision, we eliminate the successive image of the same retina. In this case therefore the part of the object that falls upon the central parts of the retina or upon its peripheral part are not completed and controlled by the simultaneous or successive images of it, the principal condition of a normal sensation. This stop of a regular progress of the visual function, suppressing the mutual compensation of images, is the principal cause of inversion.

It is clear now that a certain position of the eye is sufficient to produce one illusory form, the necessary correspondence between the parts of object and of the retina being given. But if we wish to pass from one to the other illusory form, it is

necessary that the point of the object falling in the first case upon the central part of the retina should fall in the second upon the peripheral part and vice-versa. And this can not appear without the help of movement of the ocular globe or of changes of accommodation. For this reason it is sufficient to close the eye and to immobilize it to perceive the real-illusory inversion, but it is necessary to change the accommodation or the position of the eye if one attempts to pass from one to another illusory form. But it is absolutely indifferent to the eye what means are chosen for producing inversion of a double form. It may be a change of accommodation, such as Necker and Loeb employed, or such reflex changes as were the case in our ophthalmometrical experiments, or the movement of the ocular globe used by Wundt. This is of no importance: any proceeding is acceptable which facilitates the passage of the rays from an object once on the central part to another on the peripheral part of the retina. It is indifferent to the eye again that this change is produced in a passive way, changing the relation between the object and the eye, or in an active way, by placing the eye in a necessary condition.

Three capital facts make for this theory: (1) The inversion appears even when the ciliary muscle is immobilized with atropin (experiment made by Loeb). (2) The inversion can be summoned binocularly every time that the eyes are in the condition to observe simultaneously two images. (This experiment was made by observing a remote point of a small chair made of copper wire. Two chairs were seen together with an inverted image.) (3) The duration of the illusory image that persisted notwithstanding the changes of accommodation or movement of the ocular globe.

Finally, this theory explains all the phenomena characteristic of inversion. (*a*) The rapidity of inversion that was graphically measured was, as is now easily shown, a measure of the passage from one to the other illusory form. And because this passage does not appear except through a movement of the eye or of a change of accommodation this rapidity consequently is proportional to the rapidity of these movements or to this change of accommodation. In reference to inversion in figures of three

dimensions the rapidity ceases to be characteristic, because the illusory image in this case can attain any duration.

(*b*) Again the duration of the image can be explained without difficulty. Because the inversion of an illusory form consists in a kind of stopping of the normal function of the eye, there is no reason why it should not persist as long as this irregularity lasts. In accordance with this, if one closes one eye to call up an inversion in a plane figure and endeavors to stop the movement of the eye necessary to evoke the passage of one to another illusory form, he obtains a durable inversion in the plane figure again.

(*c*) In reference to the typical movement and inclinations of the illusory image it is clear after what has been said, that successive positions of the eye, produced passively by the movement of the head without change of relation between a certain part of the object and the central and peripheral part of the retina, involve a continual change in the perspective of the retinal images. There is produced in the eye, under the influence of these successive displacements of the retinal image, a phenomenon nearly identical with what appears in such apparatus as the stroboscope or the cinematograph.

The only difference between these two phenomena consists in this: the stroboscope, being in possession of perspective images of an object in motion that were photographed from a central point but in different radii of the circle, supplies the means necessary to call into consciousness the synthesis of these images. But the images that are produced successively in the retina while the eye is displaced in different points of the periphery of the circle by the movement of the head, are, so to speak, successively photographed at different angles for the retina to produce a synthesis of an object in movement. Accordingly we can say the eye represents in this condition a living stroboscope.<sup>1</sup>

<sup>1</sup> The MS. of this article was received November 10, 1905. — ED.



